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EDITED BY

T. A. A. BROADBENT, M.A.

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T. A. A. BROADBENT, M.A.

62 Coleraine Road, Blackheath, London, S.E. 3

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THE REFORM OF MATHEMATICS.

By F. B. Pidduck.

L'étude approfondie de la nature est la source la plus féconde des découvertes mathématiques. Non seulement cette étude, en offrant aux recherches un but déterminé, a l'avantage d'exclure les questions vagues et les calculs sans issue, elle est encore un moyen assuré de former l'analyse elle-même, et d'en découvrir les éléments qu'il nous importe le plus de connaître, et que cette science doit toujours conserver. Fourier.

When Kim's lama visited the curator of the museum at Lahore, he said, "For five—seven—eighteen—forty years it was in my mind that the old law was not well followed, being overlaid, as thou knowest, with devildom, charms and idolatry."

"So it comes with all faiths."

"Thinkest thou? The books of my lamassery I read, and they were dried pith."

That Buddhist theology travelled far from the Eightfold Path is an impression which some readers may gather from Dr. Streeter's moving book: but is a mathematician in any position to throw stones? Or is his own house fragile? Perhaps he might let in some fresh air by breaking a few windows. Or perhaps (taking a leaf out of the book of the hellenic propagandists) it is safer (if less honest) to pretend that all the branches of our science are equally valuable. It is a thankless task to say that anything needs reform, when its defenders can always play to a gallery of startled bigots. Poincaré held that the theory of transfinite numbers was rubbish, and evoked the easy comment that this was an instance of blindness towards a great discovery. Fortunately, a complacence proof against external attack is sometimes disturbed from within. A writer to whom heaven and Hellas are synonymous once said that a certain Greek of old was not always truthful. He received a letter from Athens.

His correspondent wanted proof that there were *any* Greeks who were in the least degree untruthful, so he had in self-defence to search the classics in order to compile a list of Greek liars.

The first obstacle which we encounter is that as the human mind is limited, extension implies excision, and everything, however pernicious, has always someone who wishes to preserve it. The advance of mathematics depends in the long run on broadening its basis, so that ideas of more and more importance may be included, while their elaboration is kept within bounds. If the history of mathematics has any lesson to teach, it is surely that the extreme development of a branch containing only a limited stock of ideas is an unhealthy growth, and that progress has been made at intervals by infusion of new ideas, treated at first with the utmost simplicity.

Judged in this way, the different branches of mathematics differ widely. The range of ideas of the theory of functions of a complex variable and its applications (*e.g.* to algebraic functions and to differential equations) is incomparably greater than that of the convergence of infinite processes, which is still pursued with great assiduity. Investigations in the theory of trigonometrical series seem to follow a law of diminishing returns. There are enough researchers in pure mathematics in all conscience, and I am well aware of the difficulty of breaking new ground. But some of these researchers seem to pursue a subject mainly because they can write at length about it. Like Sir Walter Raleigh, they probably know very well what they are about. But if they are true students, they should pause and ask themselves what they are finding out, and if they conclude that it is not of great value, they should take to heart the words of Fourier.

" Not of great value." In matters which are not purely practical, every judgment concerning value is subjective, but that does not give everyone an equal say. We need not bow before the expressed or unconscious belief of *every* worker (not an axe grinder or lover of publicity) that his own subject is important. We need not admit that the researches by which the date of the foundation of Nineveh were put back (or forward) by four years are of the same value as those of Faraday. So I say, expecting rather to be kissed by Lord Rutherford of Nelson * than by Professor Hardy, that the properties of integral numbers are no more worthy of attention than a common puzzle. If God created the integers, He must have had other help in the manufacture of those who think of nothing else.

The theory of the convergence of infinite processes has gone far beyond its legitimate purpose of protecting those who work with

* But see the following extract from the *Mathematical Gazette* for January, 1640:

YE PILLORIE

Lord Rutherford, J. Chadwick and C. D. Ellis, *Radiations from Radioactive Substances*, p. 200.

"Column VII shows that $N \times A^{2/3}$, where A is the atomic weight, is constant for the elements examined, i.e. that the scattering per centimetre air equivalent is proportional to $A^{3/2}$."

series and integrals from error. A person who uses Fourier's series, and does not know of the principle of uniform convergence, is taking a risk. Evaluation of integrals leads sooner or later to those in which the range of integration or the integrand becomes infinite, and the theory of the convergence of integrals is more directly helpful to the calculator than that of series. But tests for slow convergence are pursued with pathological curiosity. The series $\Sigma 1/n^2$ would be said to converge rapidly, yet 40 terms are required for four-figure accuracy. I should not be ashamed if I was alone in disliking Cesàro's means ; nor, if it comes to that, do I think that information of any value is gained from the study of continued fractions.

The principal pitfall in applied mathematics is, of course, the temptation to turn it into pure mathematics : and some of us who have fallen waist-deep into this crevasse have been fortunate enough to travel with a guide. Here, again, the subjects differ considerably amongst themselves. The mechanics of particles has become a set of academical exercises, a dreary round of inclined planes, strings and parabolic trajectories : its physical content, the realisation of the meaning of Newton's laws of motion, is small. The more it is varied by multiplication of special problems, in the vain attempt to make it interesting, the less (not more) physical does it become, nor do the beloved names of Atwood and Fletcher rescue it from its position as a primitive physical science, from which experimenters should turn to better subjects. But I am trenching on what will be discussed later, the teaching of a subject in which research has ceased. In hydrodynamics much attention is paid to the theory of frictionless incompressible fluid. Every book on aeronautics refers to it with timid reverence, daring neither to ignore it nor to use it with confidence. Lord Rayleigh said one could hardly deny that much of hydrodynamics was out of touch with reality. The same is true of electrostatics, a subject handed over, swept and garnished, to mathematicians who have never been in a laboratory.

If what I am saying seems a diatribe, let me develop the point further. The more the study of physics reveals new fields, the less need there is to cultivate the old ones intensively. The more one fishes in the waters of the Irwell, the more does one draw up dead cats. The eighteenth century may be described irascibly as an era in which every British mathematician was finding the gravitational attraction of a homogeneous ellipsoid. We need not conclude that they were all, like P. G. Wodehouse's lady, solid Ivory from the neck up. It may have been, at the time, the best thing there was to do ; and the curious thought presents itself that the excessive attention to the obvious in Greek mathematics may also have arisen from there being nothing more physical for thought to bite on. The number and variety of ideas in a subject is what determines its importance and the extent to which it should occupy research. Once the matter is fairly broached there should not be much difference of opinion as to how many ideas a subject contains. Hydrostatics contains so few that its principles are almost summed up in the

equation $\int dp/\rho + V = \text{const.}$ Electricity is so varied that the researcher becomes a grateful student, without the halo which is worn, perhaps a trifle uneasily, by workers in smaller fields. So the best subjects are not the easiest to research in. In some ways I welcome the suggestion of a friend that all the chairs of mathematics should be electric chairs. If a mathematician feels that he lacks the pioneering ability or knowledge of physics necessary to pursue one subject, let him at least consult a physicist before working on others of a more mathematical type. Otherwise he may discover only that a two-dimensional condenser with thin flat plates has a circular line of force. When one works with a physicist or engineer, a feeling of annoyance comes over one on finding that their questions do not admit of an immediate or easy answer, followed by gratitude at being taken out of one's shell. Engineers are apt to think that mathematics is a magic wand (though they cannot wield it themselves), and that a trained mathematician can do anything. A mathematician, on the other hand, knows too well that soluble problems fall into limited classes, most often of the type of a perturbation or infinitely small variation. But the collaboration is useful and will no doubt increase in the future. The ideal of a person strong enough to be his own collaborator is rare, and even Sir James Jeans must stand in our second pillory for stating that Planck discovered experimentally the distribution of energy in the spectrum of a radiating cavity.

I have gone at some length into the question of research, because that is what provides, at the top, the formative ideas of a subject which ultimately gravitate downwards and permeate the treatment of the elements in good didactic treatises. Before leaving it let me refer to a point where pure and applied mathematics meet, the question of rigour. Professor Love states that no trouble is too great to secure it, if it can be secured. Lord Rayleigh thought that a mathematical physicist may occasionally rest content with arguments which are fairly satisfactory and conclusive. It is all a question of time. Even a busy applied mathematician may investigate a matter rigorously if a slapdash process has led him into error or if some instinct warns him that he is on unsafe ground: but he can hardly expect to satisfy the thirst of the pure mathematicians who are ever, like the Giaour in *Vathek*, crying, "More!"

The main fault in exposition is a lack of courage to omit. The learner has to wade through a good deal that a trained mathematician never uses or has long forgotten. This is the "dried pith" which terrifies the intelligent student beginning to specialise for the first time. The arithmetics which deal in "practice", and the algebras which deal in "scales of notation", will make the point clear to the novice. In advanced books which are models of accuracy and balance, like Love's *Mathematical Theory of Elasticity*, much is omitted and essential completeness gained thereby. I am not unmindful of the fact that one never knows what may prove to be

important in the future. But the argument for indiscriminate retention resembles that by which a great library is urged never to refuse anything under the Copyright Act. The answer is that what is valuable is soon rediscovered, possibly in a form better suited to the application which alone proves it to be useful.

Power of exposition is, of course, a gift which varies greatly. Few can hope to be like Kelvin, vitalising everything. Even Maxwell's great *Treatise on Electricity and Magnetism*, between pages of inspiration, has varying standards. It leaves a gap which has not been adequately filled since, though Webster's book has many merits. It is more annoying than helpful to find, in a book on an advanced subject, a great parade of rigour and completeness. There is a book on linear transformations in Hilbert space, a subject of some importance, whose 600 pages present hardly anything in a form likely to encourage the casual reader. Sometimes an instinct will warn the reader that the subject as a whole is not one that he can study with profit. There is an old catch which consists in asking a mathematician, " What are quaternions? " in order to hear him say, " I've got a book on it."

The principles of text-book writing are many, and should be adjusted to the circumstances. Zindler thought that purity of method is only useful in the early stages, after which the writer should be free to tread whatever path leads most rapidly to the goal. Of wider principles, the best is that the subject should come first and that the requirements of examiners should be unheeded. There is a prevalent inversion in this respect. We all know those manuals whose preface consists of a list of examinations which the reader can pass with their aid. At the best, their writers wish to do the best for the class of students they have been teaching for many years, but have become jaded in the process. At the worst, they fit a glove to the syllabus for personal gain. Let us hope that even these are not of the class which Arago had in mind when he wrote, " Qui sème une pensée dans le champ des préjugés, des interêts privés, de la routine, ne doit jamais compter sur une moisson prochaine."

Examiners may be good servants, but they are certainly bad masters. The " dreary round " in mechanics has been generated by them. Conic sections attract them fatally, accentuating that desire to do everything by a different special method which is one of the less advertised legacies of Greece. There is no glow of enthusiasm on learning that the feet of the four normals from a point to a conic lie on a certain rectangular hyperbola, but most English books contain the information. It is, in fact (if I may develop this small point), corrupting, because the only interest of the hyperbola is as a locus in which all its points are involved. To say that four points are on it is as nearly as possible trivial. I think we have something to learn from France, Germany and America in these matters.

A well designed course of geometry should have the elements written as far as possible in current prose : nothing to be proved that an intelligent and wholly ignorant reader would not regard as obvious.

The attempts that have been made in the past have been far too timorous. Who ever thought that the angles at the base of an isosceles triangle, or the opposite sides and angles of a parallelogram, might be unequal? Then why is the subject mentioned at all? To prove the obvious is to bring mathematics into discredit, in addition to wasting time. I do not think there can be much difference of opinion as to what is self-evident. Certainly Pythagoras' theorem is not, though the fact that the angle in a semicircle is a right angle, as Rouse Ball pointed out, needs no more demonstration than a glance at a tessellated pavement with a rectangle inside a circle. In proving Pythagoras' theorem we should look to the future, and remember that the proofs of the more general theorems,

$$\cos(A-B)=\cos A\cos B+\sin A\sin B \quad \text{and} \quad \cos\theta=ll'+mm'+nn',$$

depend only on the elementary principle of projection. The proof as laid out for the beginner would be somewhat as follows. To fix ideas, let the hypotenuse be of length 1 and the other sides of length x and y. If a perpendicular is drawn from the right angle to the hypotenuse the portions cut off are of length x^2 and y^2, so that $x^2+y^2=1$. The exact form of the proof is immaterial. It might be convenient to expound the idea of projection and the cosine of an angle in advance. What is essential is to realise that proportion is a very simple idea needed in common life, and that one of its consequences is the theorem of which Euclid gave so preposterous a proof. Somewhat similar is the following proof given to me in a class in elementary mathematics at Exeter College, Oxford, of the theorem that the bisector of the vertical angle of a triangle divides the base in the ratio of the remaining sides.

Consider the two triangles into which the triangle is divided by the bisector. There are two ways in which they can be considered to have equal altitudes. In either case the areas are in the ratio of the bases.

Nothing else is wanted but an unmarked figure.

More than patching is needed to produce an introduction to geometry which shall do credit to the present age (I am not, of course, reflecting on Professor Baker's admirable work). A little quiet reflection is necessary before we can rid ourselves of the shackles of tradition, and the book should (and could) be written without reference to any other book, simply and brightly while the mind is fresh. I have not, at the moment, any considered scheme for the book which is so urgently needed. Still less have I thought of its continuation. The following are a few suggestions thrown out at random.

Vector analysis is a branch of elementary mathematics, with some of the merits and defects of synthetic geometry, and not entirely successful in its later development. Heaviside thought rightly that it should be used as soon as possible in elementary teaching: not later, certainly, than the cosine theorem of the triangle of which it furnishes the most excellent proof. Argand's diagram, introduced with the complex number, fits well into vector analysis,

and inversion is treated more naturally as the transformation $1/z$ than otherwise. Homographic transformations in Argand's diagram follow, built up from elementary transformations, inversion and three others interpreted simply as strains of a plane lamina : and hence we can pass (after coordinates have been assimilated) to the reform of the theory of homography. The way this is treated in some of the older books is enough to produce a distaste for the whole subject. Anharmonic ratio is enthroned, and everything possible done to conceal the fact that homography is a simple correlation of two variable points. The objections to this course are twofold. Remembering the four normals to a conic, let us emphasise again the enormity of slurring the distinction between a constant and a variable. To me the statement that four points $ABCD$ are homographic with four other points $A'B'C'D'$ if the anharmonic ratios are equal is anathema. The first stage in the cure is to stress the fact that D and D' are variable. The second needs more explanation. The only simple view of homography is that it is what is known technically as collineation. For every point P in a plane, we take a point P' such that when P moves on a straight line, P' does too. If we choose further to take a "section" of the transformation, and consider only those points P which lie on a given line, we have homography on two different lines (and similarly on two conics, or the same conic). Nothing could be more attractive. There are many types of unrestricted correspondence, and even the restricted correlation contains three arbitary constants. Having, therefore, got as far as saying that the anharmonic ratios of $ABCP$ and $A'B'C'P'$ are equal, what are A, B and C doing? They appear by eliminating the arbitrary constants, and so does the combination $AB.CP/AP.CB$. I would sooner have the arbitrary constants.

Freshness often outweighs experience, and everything gains by being thought of anew. How many students have ever plotted a curve in areal coordinates? If they use them at all, ought they not to do so? Will it be worth while? I offer the following as a compromise. No curve need be plotted at all. Let the student take one plotted already in cartesian coordinates, call the origin C, and mark off unit lengths CA and CB on the axes. Then the curve is ready made in areal coordinates with ABC as triangle of reference. But I must apologise for having allowed so much of this essay to deal with elementary matters.

To what has been said of examinations I would only add that the best questions are the most straightforward ones, preferably those which admit of more than one solution. The arch enemy is the examiner who seeks to narrow the schedule and raise the standard of technical achievement within it. If the position assumed in this essay is a sound one, the proper solution is found in the opposite direction. Most searching questions can be put without a trace of artifice in the mathematical theory of electricity. But whatever the subject, let us have a wide range and a liberal choice. This alone can save us from a dangerous and ever present menace. F. B. P.

ON CERTAIN RELATED CURVES.

BY R. GOORMAGHTIGH.

1. Professor C. E. Weatherburn's paper in the *Gazette*, 1936, p. 320, gives an interesting proof by vector-analysis of the condition for a twisted curve C to have as binormals the principal normals of another curve C_1.

It may be of some interest to give a few bibliographical notes on the subject and to mention new or not generally known properties of certain of these pairs of curves.

The condition for a curve C to have as binormals the principal normals of another curve is given by Schell, *Allgemeine Theorie der Kurven doppelter Krümmung*, 3rd ed., Leipzig, 1914, p. 67. Inversely, the relation between curvature and torsion of a curve C_1 having as principal normals the binormals of another curve C was already published by Mannheim in the *Comptes-rendus de l'Académie des Sciences de Paris*, 1897, 2nd vol., p. 849.

2. Let P be the current point on C, ψ the inclination of the tangent at P to a fixed tangent, when C is unwrapped into a plane, s and s_1, ρ and ρ_1, τ and τ_1, the arc-lengths, the curvatures and the torsions of C and C_1 at P and at the corresponding point P_1 on C_1.

It follows then, from Professor Weatherburn's paper, that

$$\rho_1=\frac{1}{a}\sin^2\psi, \quad \tau_1=-\frac{1}{a}\sin\psi\cos\psi.$$

Hence $$\rho_1^2+\tau_1^2=\rho_1/a.$$

Further, the central axis of the curve C_1 at P_1 is the tangent to C at P: hence, *for the curves C_1, the surface of the central-axes is developable.*

Finally, for the curves C_1, the square of the curvature of the spheric tangent-indicatrix, being $(\rho_1^2+\tau_1^2)/\rho_1^2$, is proportional to the curvature radius of C_1.

3. A curious case is that when the curve C_1 is a twisted catenary. Cesàro, in a question proposed in *Mathesis*, 1899, p. 280, had already noted that such curves are, at the same time, geodesics on certain cones; Cesàro's question asked for a geometric description of the corresponding cones, and we gave recently, *Mathesis*, 1936, pp. 207, 280, such a description.

It is well known that the arc length s_0, the curvature ρ_0 and the torsion τ_0 at any point P_0 of a geodesic C_0 on a cone satisfy Enneper's condition $b\tau_0=\rho_0 s_0$, the parameter b being the distance ΩS from the vertex Ω of the cone to the straight line into which C_0 is transformed when the cone is unwrapped on one of its tangent planes; S will be denoted as the vertex of the geodesic.

If Q_0 is one of the points where a generator ΩP_0 (P_0 being on C_0) meets the sphere Σ, having Ω as centre and b as radius, and if s_2 and

ρ_2 be the arc-length and the curvature of the locus (Q_0) of Q_0 at Q_0, it is easy to show that

$$b^2\rho_2^2 = 1 + b^2\rho_0^2 \sec^6 \frac{s_2}{b}, \quad s_0 = b \tan \frac{s_2}{b}.$$

When a curve C is a geodesic, with parameter a, on a cone, *i.e.* when

$$\rho_1^2 + \tau_1^2 = \rho_1/a, \quad a\tau_1 = \rho_1 s_1;$$

then

$$\frac{1}{\rho_1} = a + \frac{s_1^2}{a}.$$

The curve C_1 is then a twisted catenary having its vertex at the vertex of the geodesic, and the spherical curve, analogous to (Q_0), has for one of its intrinsic equations

$$a^2\rho_2^2 = 1 + \sec^2 \frac{s_2}{a}. \tag{1}$$

But, if a curve Γ with a constant torsion $1/a$ is drawn on a sphere having a as radius, the well-known relations between the intrinsic elements of any spherical curve will easily lead to an equation of the form

$$\rho = \frac{1}{a} \sec \frac{s}{a},$$

and one of the intrinsic equations to the spherical indicatrix of the binormals of Γ on the given sphere will be analogous to (1).

Hence, *if a twisted catenary, with parameter a, is a C_1 curve, it is a geodesic with parameter a on a cone formed by the parallels drawn from a point to the binormals of a spherical curve having a constant torsion radius equal to the radius of the sphere on which that curve is drawn.*

4. It is, further, easy to prove that the square of the curvature of the inverse curve to the geodesic, considered in Section 3, with respect to the sphere Σ, is

$$\frac{4}{b^2} + \frac{(b^2 + s_0^2)^2}{b^4} \rho_0^2.$$

When the geodesic is the considered twisted catenary, this expression is constant.

When C_1 is a twisted catenary, geodesic on a cone, the inverse curve to C_1, in an inversion having the vertex of the cone for pole, is a twisted circle.

5. In this case, the corresponding curve C is also an interesting one.

Since the tangent to C at P is parallel to P_1S, the plane formed by that tangent and by the binormal P_1P to C at P passes also through the fixed point S, and C is then also a geodesic on a cone, having S as vertex and a as parameter.

More generally, let C be a geodesic of parameter b on a cone; then, if t and n are the unit-vectors in the direction of the tangent and the

principal normal at P, and t_1 the unit-vector in the direction of the tangent at P_1, and if $\tan\theta=\frac{a}{b}\rho s$,

$$t_1=\left(t-\frac{a}{b}\rho s n\right)ds/ds_1=t\cos\theta-n\sin\theta;$$

the unit-vector in the direction of the principal normal at P_1 will be parallel to the binormal at P when $\rho+d\theta/ds=0$. Hence $\theta=-\psi$ and

$$\cot\psi\, d\psi=-\frac{b}{a}\frac{ds}{s}.$$

Therefore, c being a constant,

$$s=c\sin^{-\frac{a}{b}}\psi.$$

When $a=b$, *i.e.* when C_1 is the considered twisted catenary, *the curve C is obtained by twisting the evolute of a catenary of uniform strength.*

When $a=-b$, then $s=c\sin\psi$; *amongst the C curves, geodesics on cones, there are twisted cycloids.*

In this case, the curve C_1 is the locus of the image through P of the projection of the vertex of the cone on the binormal of C.

R. GOORMAGHTIGH.

GLEANINGS FAR AND NEAR.

1144. Mr. R. C. Morrison in the House of Commons, April 22, 1937, called the Chancellor of the Exchequer a " Gradgrind " Chancellor, defining Gradgrind as " one who regulates all human things by rule of compass and mechanical application of statistics, allowing nothing for sentiment, emotion and individuality ". [Per Dr. J. Wishart.]

1145. Now mathematicians constantly reach valuable results by making use of symbols, such as $\sqrt{-1}$, to which nothing in human experience is known to correspond.—Canon B. H. Streeter, *Reality, a new correlation of Science and Religion*, p. 12. [Per Mr. Frank Robbins.]

1146. From the 1833 edition of Welch's *Improved American Arithmetic*, a text-book widely used in the United States in the middle of the nineteenth century :

If a Cardinal can pray a soul out of purgatory by himself in 1 hour, a Bishop in 3 hours, a Priest in 5 hours, a Friar in 7 hours, in what time can they pray out 3 souls, all praying together?

In the 1842 edition, this question is replaced by :

If a steam engine can, working alone, do a certain amount of work in 1 hour, a water-mill in 3 hours, a wind-mill in 5 hours, and a horse-power mill in 7 hours, what time will it require for the whole, working together at the same time, to do three times the amount of work? [Per Mr. F. J. Wood.]

1147. On Saturday I could scarcely believe my eyesight; the ball described angles and bisections not known in my geometry.—Neville Cardus on the Melbourne Test Match, *Manchester Guardian*, January 5, 1937. [Per Mr. C. E. Kemp.]

THE TEACHING OF ELEMENTARY ASTRONOMY.*

Mr. J. A. Edgar : On beginning the study of astronomy it is imperative for the student to gain familiarity with the brightest stars and with the classical constellations and their relative positions. The stars apparently all shift their positions westward during the night, and there is further a seasonal drift due to the annual revolution of the earth about the sun, so that a permanent acquaintance with the principal groups must depend on an intimate knowledge of their relative positions. In imparting this knowledge it is tedious and inefficient to point out the groups to the student, and published star maps are usually confusing, as they present too many configurations at once. Much can be gained by the student constructing his own maps. In this way the star chart grows, gradually extending familiarity with the sky. The first chart to construct is that showing the circumpolar group of stars. For this purpose polar graph paper must be used, and coordinates analogous to latitude and longitude are adopted. The coordinates of the stars (Declination and Right Ascension) are taken from the *Nautical Almanac*, and this " book of words " ought to be used from the start of the subject. This map also serves to introduce the use of the heavens as a clock, identifying sidereal time with the motion of a definite line in the sky, a great circle passing through the pole and β Cassiopeiae (approx.). This map is also used as a basis for observation with a small telescope, *e.g.* the positions of the star clusters in Perseus can be located and examined. If a suitable minimum in the light intensity of Algol occurs during observation times, the student can spend a profitable evening watching the light fluctuation of a binary system of stars. Even the smallest of instruments or a pair of binoculars reveals a great wealth of observational material in this part of the heavens.

The equatorial regions can be plotted on ordinary graph paper, but it will be found profitable to arrange an overlap with the circumpolar map. Two maps of this region, between the equator and middle latitudes of the heavens, should be made, one for the Summer and Autumn Constellations (Boötes, Hercules, Lyra, Perseus, Aries, etc.), and the other for the Winter and Spring Constellations (Perseus, Orion, Taurus, etc.). These maps again serve as the basis of considerable addition to the knowledge of observational astronomy. The colours of Vega and Arcturus can be examined and the multiplicity of ϵ Lyrae revealed with a pair of binoculars. In fact so rich is the material in this part of the sky that it is necessary to have enlarged maps of certain of the constellations, *e.g.* of Orion.

In all of these maps the brightness of the stars should be indicated in some way, and reference books containing lists of suitable objects for small telescopes will be found indispensable.

This gives a brief synopsis of the most elementary part of the

* A discussion at the Annual Meeting of the Mathematical Association, 5th January, 1937.

work and at the end of it the pupil is ready to assimilate new ideas. He has gained a fairly clear knowledge of elementary stellar motions without much reference to the earth, and is now in a position to appreciate more fully other systems of celestial coordinates. From the point of view of the surveyor, the basis of accurate measurements of position on the surface of the earth is the determination of latitude and longitude. This involves finding the position of the celestial pole relative to the observer's horizon and can be introduced by reference to a very simple experiment. Using a simple laboratory gnomon and a hemisphere to represent the part of the heavens seen by the observer, a series of observations of the altitude and azimuth of the sun can be taken and the diurnal arc plotted on the hemisphere. A more detailed explanation will be found in Stetson's *Manual of Laboratory Astronomy* (Eastern Science Supply Company, Boston, Mass., U.S.A.), where other simple experiments are also described. In place of the hemisphere an ordinary glass bulb half-filled with liquid can be used. Of course if any better equipment is available the azimuth-altitude determinations can be made for any celestial body, and if a small telescope which can be clamped in the meridian is available a still more accurate determination can be made. (See any textbook.)

This experiment ignores the sun's progressive motion throughout the heavens. To show this, a celestial globe can be used or a home-made sphere could be adapted for use in conjunction with the *Nautical Almanac*. Without using either of these pieces of apparatus, the sun's path can be plotted on the original maps, taking monthly intervals in Declination and Right Ascension and marking the dates. The inclination of the ecliptic to the equator and the full significance of the vernal and autumnal equinoxes can now be deduced.

Before proceeding much further in the subject the student requires a more intimate knowledge of sidereal time, and the relationship of sidereal time, apparent solar time, and mean solar time. He has already learned to associate sidereal time with the motion of a particular circle in the sky, and to consider sidereal time as the Right Ascension of any object on the meridian (from the circumpolar chart). The adoption of the sun instead of the vernal equinox as time-keeper gives rise to apparent solar time, the eastward progression of the sun making the solar day about four minutes longer than the sidereal day. It is essential that the student should grasp this point, and there is no better method than the use of a celestial globe to answer such questions as these: What is the hour angle of Rigel when setting at a place of latitude 51°, and what is the corresponding sidereal time? What is the local apparent time of the setting of Rigel at monthly intervals throughout the year? This necessarily leads up to problems on the mean sun, a fictitious sun which moves along the celestial equator at a uniform rate, and a repetition of the gnomon experiment or its equivalent can be used to show the difference between mean time and apparent time (the equation of time).

In order to show this more clearly it is instructive for the student to plot the Equation of Time from readings in the *Nautical Almanac*. At this stage nothing is gained by a more intimate discussion of the sun's motion. By use of a globe or the *Nautical Almanac* the student can now calculate local civil time from sidereal time and *vice versa*.

Two experiments may be mentioned here. Firstly, the longitude may be determined by using a clamped telescope or by means of a gnomon (length of longest shadow and equation of time). Secondly, the student may design a horizontal sun-dial which can be used in conjunction with the equation of time. If a celestial globe is available, the design of this instrument can be carried out without making any trigonometric calculations. (See Stetson's *Manual of Laboratory Astronomy* for further elucidation of these points.)

Naturally the student does not spend all his time following a logical course of instruction. His attention is distracted by looking at planets and the moon through a small telescope, but I am not going to deal with this topographical side. By some very simple experiments, conducted necessarily over a long period, valuable information of the moon's orbit can be obtained. This can be done by using a simple cross-staff to determine the angular distance of the moon from several stars and so plot its position on a star map. The position can, of course, be determined directly from the *Nautical Almanac*, and probably a combination of the two methods would be better for teaching purposes. If we plot the moon's position among the stars over a fairly long interval its period can be found, and by plotting the corresponding positions of the sun (from the *Nautical Almanac*) its galactic longitude is at once explained and determined. The student can then draw his own diagram explaining the moon's phases. Other information obtainable from this map includes the moon's sidereal and synodic periods and verification of the equation $I/S = I/M - I/E$. (See textbooks.) The inclination of the moon's orbit to the ecliptic can also be determined. The cross-staff method can be extended to measure longitude.

Various other graphical exercises on planetary theory will be found in Flammarion's *Annuaire Astronomique et Météorologique*.

This represents only a small part of what can be done with the young student. It has necessarily been very condensed, and full explanations have not been given. Useful practical information and illustrative diagrams of this type of work will be found in books such as that of Stetson already mentioned. Most of the work there described can be carried out with simple home-made apparatus, and for much of it no telescope is necessary, although a small instrument is extremely useful.

Dr. L. E. Lefevre (Eton) : I should like, in the time placed at my disposal, to make a plea on behalf of a simple course in astrophysics in the curriculum of boys in the specialist stages of their school careers. I fear this may strike a rather ambitious note, on first consideration, but it is not long since I was perusing an article from your *Gazette* in which the author mapped out a course for mathe-

matical specialists embracing such topics as Dedekind Sections and the Heine Borel Theorem. I hasten to assure you that I do not propose to champion such profundities to-day. There will, indeed, be little, if any, of mathematical work in the topics I suggest for consideration. But as a complement to a mathematical specialist's course there must surely be a great deal to commend itself in the story that has been unfolded of the physical characteristics of the heavenly bodies, and of the architecture of the universe, since Huggins first had the audacity to attempt to photograph the spectrum of a star some sixty years ago. The fact that that bold experiment laid down the foundation of the imposing edifice of knowledge that we have to-day, including answers, more or less satisfactory, to such questions as "What are the stars made of?", "How hot are they?", "How luminous are they?", "How far away are they?", "How dense are they?", "How fast are they moving?", and even perhaps in some cases "How big are they?", must suggest that here surely is something to absorb the interest of a genuine student with a mind bent towards scientific curiosity, something that will stir his imagination towards something beyond mere "bread-and-butter" utilitarianism, and give him a taste of intellectual activity pursued for its own enlightening sake.

Having delivered myself of my apologia, it is natural to ask what sources of material are available for such a project. To many people a passing acquaintance with the broad outlines of modern astrophysical progress is given through the popular expositions contained in the series of books by Sir James Jeans, such as *The Universe Around Us*. As an initial stimulus these might prove an excellent introduction, but I cannot but feel that there is a real opportunity for stimulating students' minds to a point where they wish to find out a good deal more about the methods by which these results were obtained. I do not know that I should recommend the use of a textbook for such a purpose, except in so far as one would be required for reference to illustrations, etc. Russell, Dugan and Stewart's *Astronomy*, Volume II in particular, would certainly be a most valuable ally in this respect, and much of the text is well within the scope of an intelligent pupil. But the rate of development of astrophysics during the years in which it was being written, and subsequently, has been so rapid that it can by no means be regarded as up to date, and a great deal of stimulating, supplementary material might be dug out of less accessible sources, such as the series of Harvard Observatory Monographs, *Handbuch der Astrophysik*, or Professor E. A. Milne's Bakerian Lecture to the Royal Society in 1929, or yet from the multitude of original papers in the *Monthly Notices of the Royal Astronomical Society* or the *Astrophysical Journal*.* All this, of course, presumes, as I well know, access to a suitable scientific library, and the expenditure, on the part of the teacher, of a great deal of time and labour—probably

* Note added in proof: Professor Russell's long series of articles in the *Scientific American* make excellent material for talks on these topics.

the sacrifice of a fair portion of his well-earned holidays. But I am prepared to hold this in favour of the scheme rather than as opposed to it, for as far as the teacher himself is concerned he will find his time not ill-spent, the change from his normal routine may help him from settling too deeply into his pedagogic groove. Further, if he can communicate to his class the sense of having gone outside the usual schoolbook channels for his material, there may be fostered between teacher and taught together a mild sense of the thrill of the research worker. The fact that the subject is all so very new provides the student with the opportunity for feeling himself following closely behind the footsteps of the pioneers, thus adding very materially to the charm and stimulus of the subject.

Lantern slides would prove, of course, an invaluable aid to such a course. By far the best available are those published by the Mount Wilson Observatory and the Yerkes Observatory of the University of Chicago, for they are actual reproductions from exposures with their own instruments, the finest in the world at present.

Time does not permit me to suggest more than a sequence of chapter-headings that might be developed along such lines as the teacher's own enquiries lead him. Although to some of you it may sound strangely unfamiliar ground to think of treading, I am convinced that every subject-heading I shall mention is capable of being treated, at least in a descriptive way, well within the grasp of the ordinarily intelligent specialist.

I. Spectra in the Laboratory.

Means for producing spectra. A general description of continuous and line spectra. Atoms and electrons. The Rutherford model. Simple Bohr theory of the hydrogen line spectrum. Stationary states and electron jumps. Series of lines. Degrees of excitation. Ionisation. Arc, furnace, and spark spectra. Effects of temperature and pressure. Line-width. Absorption spectra.

II. Stellar Spectra.

Photography. Types and classification. Problems of the qualitative analysis of stellar atmospheres. Conditions of excitation.

III. How We tell the Temperatures of the Stars.

(*a*) From the degree of excitation or ionisation indicated by the relative strengths of the lines present in the spectrum.

(*b*) By colour. Direct photography, using filters. Photometry of continuous spectrum.

IV. How We measure the Candle-powers of Stars.

Giant and dwarf spectra. Mount Wilson criteria for absolute magnitude. The Russell diagram.

V. How We measure the Distances of Stars.

Trigonometric parallaxes.

Comparison of real and apparent brightnesses. Inter-stellar lines.

VI. How fast do the Stars move?

Proper motions.

Doppler's Principle. Radial velocities.

VII. Special Types of Stars.

Binaries, visual, eclipsing, spectroscopic.

Variable stars. Cepheids. Period-luminosity law.

VIII. Clusters and Nebulae.

Globular clusters and their evidence as to the size and shape of the galaxy.

Galactic nebulae and their evidence as to the history of the galaxy.

Extra-galactic nebulae. Determinations of distance and size. Speeds of recession. Hubble's velocity-distance relation. The expanding universe.

I am afraid that this bald catalogue cannot sound anything but uninspiring to anyone not familiar with the jargon, and I can only add to it my own testimony that there is considerable intellectual satisfaction to be derived from delving into some of these topics somewhat carefully, by anyone sufficiently keen about it to give the time to it.

Perhaps I may be permitted to conclude with some remarks on a philosophic aspect of the subject. I have suggested this course as suitable for students aged about seventeen, an age at which the first hints of a genuine philosophy of life may be in process of formation. It may be regarded as unwise to encourage such impressionable minds to dabble in these vast conceptions of size and distance, so strongly calculated to focus vivid attention on man's physical insignificance in the natural universe. Surely here, you may say, is dangerous material for the foundation of distorted views on values in the life ahead, food for potential cynics, defeatists, or amoralists. The good teacher will readily avoid this attitude, and succeed in measuring, not man's physical stature against the yardstick of galactic dimensions, but his intellectual stature in terms of the magnitude of the problems he has set himself to solve, and of the knowledge he has gleaned from the deciphering of the broadcast messages from the distant stars and nebulae. The story of man's achievements in the field of astrophysics during the last half-century form as thrilling a tribute to man's intellectual greatness as can be found, and it is my contention to-day that it is one that can, with a certain amount of preliminary spade-work, be brought home to the type of pupil I have suggested.

Mr. R. L. Marshall: There are, and I suppose always will be, two schools of thought in the educational world. The first is what I may call the *aller de penchant* school of thought, which is for allowing the student to choose his own line of study, as far as possible, and to follow only those branches of learning for which he has—or thinks he has—a natural aptitude. Why, it is asked, should a student be

required to cram up some dead language, or even modern languages, when his natural bent is in the direction of science? Why should he be bothered with the dull facts and figures of science when his natural bent is towards something more romantic or aesthetic? Why should he be called upon to apply himself to the study of geography when the faculty of history is more to his taste? Or to weary himself with committing to memory historical dates when his natural bent inclines to the study of geography? It will, presumably, be granted that no branch of learning can be pursued effectively unless one can read and write, but otherwise this doctrine, pushed to its logical extreme, would make the public schools and universities a sort of educational Woolworths—everybody to go in and pick out just what he likes.

The objections to this doctrine are so many that the danger is of not seeing the wood for the trees. To mention one of them: there is a danger of the student taking up some branch of study for which he has not even any special aptitude at all, under a mistaken notion of following the line of least resistance. For instance, he (or she) may think that astronomy can be taken as a soft option instead of taking a second foreign language. Then there is the opposite school of thought, which aims at prescribing uniform bases of culture even at the risk of making the scholars pursue uncongenial tasks—even at the risk of forcing a round peg into a square hole. It is a poor achievement, it may be argued, to persuade them to follow their natural bent. It is one thing to drive the willing horse, but it would be much better horsemanship to break in the wild horse.

I have two statements to make with regard to the learning of astronomy, both based on my own experience—and if other people's experience differs from mine, then let us know it.

The first statement is that the study of astronomy is not popular at schools, and the second is that those few who have a taste for what is vulgarly called "star-gazing" are the least likely to pursue the study of astronomy with any advantage or profit.

Any person who wants to make the study of astronomy compulsory must be prepared to advocate compulsory classical languages. There are risks incidental even to the voluntary study of astronomy in schools, but these will be found incidental to all or most optional subjects. For instance, the taking of a scientific subject is semi-compulsory for the Oxford Responsions. By that I mean that there is a compulsory option to take either mathematics or science, or both.

I have here no first-hand knowledge, and cannot give the actual facts and figures but, by inspecting the Ordo Respondentium and the lists of those who have passed in three subjects or in one subject at a single examination, I have been led to the conclusion that some candidates have been induced to take science with the idea of following the line of least resistance, and because it may seem to offer a soft option as an alternative to mathematics. It will be a bad day for the study of astronomy if candidates for an examination are

induced to take it as a soft option instead of mathematics or a second foreign language.

Dealing with individuals, I should say that a person with an aesthetic or poetic taste for astronomy had better leave the subject alone. I am quite certain that a poet who can compose

> "... the Pleiads rising thro' the mellow shade
> Glitter like a swarm of fire-flies tangled in a silver braid"

will never attain to any degree of proficiency in the study of astronomy. None the less, I do not despair of the teaching of astronomy in schools.

The chief thing to remember is that the subject of astronomy cannot be kept in a watertight compartment. The faculty of astronomy does overlap with higher mathematics, mechanics, geography and geology—and some at least of these branches of learning are taught as part of the regular curricula of practically all schools, especially geography.

It should also be remembered that a practical knowledge of astronomy is necessary to sailors for the purposes of navigation. To tell the latitude and longitude and to make use of the chronometer require a certain knowledge of astronomy, which in this department at least cannot be regarded as merely an abstract science.

What I am more particularly concerned with here is the common ground on which the science of astronomy overlaps those of geography and of geology—and that common ground is what I call the astronomy of the Earth. It is already required that students of geography should understand the causes of day and night, of the seasons of the year, and of the tides. Geology requires that they should understand the great epochs of the world's history, including the glacial epoch or Ice Age.

But in all these branches of learning in which astronomy invades —or is invaded by—the sciences of mathematics, mechanics, physics, geography and geology, there is one very important thing on which the attention of all teachers and students should be directed. That is to make themselves masters of the facts and figures and to concentrate on the dull drudgery of the work. If genius is known as an infinite capacity for taking pains, the importance of concentration on the dull drudgery of the work will be self-evident—but it is not always self-evident.

I have already said that poetic imagination will never make an astronomer. It will certainly prevent the making of an astronomer if it is allowed to be imported into his scientific work. The science of astronomy must necessarily consist of a mass of dull, dry facts and figures, to be correlated for the purpose of arriving at scientific conclusions.

The student will have learnt something when he knows that the diameter of Mercury is 3000 miles. If he can tell the exact number of miles and the difference between Mercury's polar and equatorial diameters, he will have learnt much more. Whenever he can find out the period of Mercury's diurnal rotation, he will have contri-

buted much to astronomical science. Perhaps this is rather off the point. I only mention this because Mercury is the first planet in order from the Sun, whereas we are mainly concerned with the astronomy of the third planet in order from the Sun, namely the Earth. These things belong to the astronomy of the Earth: the diurnal rotation of the Earth, the causes of the seasons, the difference between a sidereal and a solstitial year, the obliquity of the ecliptic, the second rotation of the Earth and the precession of the equinoxes, the possible causes of a glacial epoch, the action of the Sun and Moon in producing the tides.

To sum up: the chief danger in the teaching of astronomy at schools is lest it should be taken as a soft option under a mistaken idea of following the line of least resistance. A somewhat heroic remedy for this would be to make the learning of astronomy compulsory, in which case the inclinations of individuals need not be considered. There is one other danger which is inherent in the study of astronomy; that is, of thinking in geocentric terms, *i.e.* of thinking as if the Earth were the centre of the Universe. Now, in studying the astronomy of the Earth, this danger can hardly be said to exist.

As it is, I have one constructive proposal to put forward, and that is that the astronomy of the Earth, instead of being taught piecemeal as parts of other faculties, should be taught as a separate sub-faculty and examination papers set accordingly. The advantages of this would be twofold. (1) It would aid the teaching of the other faculties, and (2) it would enable the masters to sift out those pupils who have and those who have not a genius for astronomy itself, so that the former might, with advantage, pursue astronomy in its higher branches.

The Chairman (Mr. Siddons): We have listened to three papers ranging from elementary work that might be done in any school, almost by any child, to an aim which will appeal to some who deal with mathematical specialists. I imagine that Dr. Lefevre would not expect every man dealing with VIth Form specialisation to take up the subject as he has suggested, but to some, no doubt, that would appeal very much, and he has been very suggestive from that point of view.

Mr. W. F. Bushell (Birkenhead): I have had many years' experience of teaching astronomy at schools, sometimes with the use of a 6-inch refractor in what I might call a proper observatory, sometimes with a 3-inch refractor on a stand, and sometimes with no refractor at all. I have taught it both in the Northern and the Southern Hemispheres. I am afraid I cannot pretend to have achieved much mathematical success. Mr. Edgar quoted a professor as saying that astronomy is a hard taskmaster. I entirely agree. For most of us it is, I think, very difficult to achieve much of what I might call mathematical success in astronomy.

Then there is, of course, the time difficulty. That has always clouded my efforts, and I suppose it will always cloud everyone's

efforts. It is not so much the putting in of a new subject. Rather it is that if you have an observatory under your control and go to it with boys, and get everything ready, the dome open, and all the rest, clouds may well come along, and you have thus wasted a considerable amount of time. That is probably why the criticism is so often urged that school observatories, when they exist, are insufficiently used. I believe that criticism to be true.

Mr. Marshall said that astronomy as a subject in schools is rarely popular, and he invited criticism of his remarks. I desire to challenge that statement very strongly. I think there is no school subject—actually it is not strictly a school subject—which is better liked if presented to the boys in its more popular aspect. I have made an attempt, at different schools, with a substantial number of boys in perhaps three different ways. Firstly, a systematic series of seven or eight popular lectures with slides, given week by week; secondly, I have tried to make them gain a knowledge of the constellations and better known stars by observation outside on fine nights; and thirdly, if I have had a telescope available, I have told them that there are at least six things they ought to see: Jupiter, Saturn, a sunspot, the lunar surface, a nebula and a double star. If I can show the average boy these objects through a 6-inch refractor, or even a smaller telescope, I feel I have accomplished something.

I was much struck by the remarks at the opening of the discussion with regard to the making of star maps. I should imagine that there is no more fertile way of teaching a boy the sky than by something of that sort. My efforts have been mainly confined to observation work with a planisphere or star map, but what I teach in that sort of way is probably generally forgotten! I do not doubt that it is far better for a boy to make his own maps of the sky.

It has been said that there is danger of the subject being regarded as a soft option. I wonder? I had the privilege many years ago of founding an astronomy prize in a well-known public school, which prize is still annually awarded, and certainly in its early days it achieved substantial success. Indeed, one of the winners shortly after leaving the school, and going into ordinary civilian life, used to go out with a pair of field-glasses, and finally discovered a nova, which I think was rather striking in that it was a nova which had not previously been discovered by any of the great observatories of the world.

May I conclude by saying that although I believe as a definite school subject we can put in very little astronomy in ordinary school work, my plea would be that what we do put in should be on a rather more systematic basis than hitherto.

The Chairman (Mr. Siddons): When I first went to Harrow I had the use of a very good 6-inch telescope. It was in a somewhat isolated position, and it took a good deal of time to get there, and I had trouble on account of clouds. On one occasion Professor H. H. Turner came to stay with me. I described to him the work we were

doing, and he strongly advised me to get the telescope moved into my own garden so as to have it very near, in spite of the fact that it meant a limited field over which we could observe. He said we could do much more if the telescope was nearer to us, but he advised me not to attempt to do more in its present position, saying, "If you want to do twice as much, you will have to give ten times as much time to the work." He realised that we could use the telescope as a means of stimulating interest.

Another experience I had was in connection with taking boys specialising in geography. I was put on to teach them some mathematics and, not unnaturally, drifted off into astronomy. I did a good deal of work rather on the lines Mr. Edgar has suggested, not sky work but Earth work. I used to get some interesting questions over map projections and plotting on different projections the various paths, drawing the path on one projection and transferring it to another. I hope it will be possible to have Mr. Edgar's slides reproduced in the *Mathematical Gazette*, and to have there a further account of the work he is doing. Mr. Edgar did not say whether he would like to see astronomy introduced generally or as a hobby for the boy who would work at it out of ordinary school time.

Mr. Edgar : I do not take astronomy up as a school subject at all ; it is taken up purely as a hobby, and I have no difficulty whatever in maintaining the interest of about forty boys from the College, which is as many as we can accommodate in the observatory during term.

I realise that Mr. Bushell's criticism is rather vital, but I think the trouble lies not so much with the subject as with ourselves. It is necessary to supply, as I said before, a course which is suitable for a school ; that is to say, a course that can be carried through no matter what the weather conditions are. I am afraid that we most of us allow our minds to run on the more advanced side of the subject. We tend to think in terms of Jeans's books and in that way fail to realise the importance of the elementary side of astronomy and the importance of getting out a syllabus. Even for a vocational subject I find that most necessary, and the syllabus must be very elastic ; that is to say, you must have one that you can apply on a wet day or take the opportunity to observe as it occurs. These two vital facts are by no means incompatible, although it has taken me a long time to develop a course which is at all satisfactory.

Dr. Lefevre emphasised the side which, I agree, is suitable for boys about seventeen. I do something along those lines as well with boys in an astronomical society, in which we discuss these problems, and I also put some of the boys on to what might be called research work. I have one or two just now plotting the orbit of satellites of Saturn by means of a chronograph which we made. The main observation work with junior boys is purely looking at things and, as I said, drawing their own charts and getting their own ideas instead of taking them straight from books. Our book of words is the *Nautical Almanac*, which the boys begin to use at fifteen.

Mr. R. L. Marshall: There is only one remark I have to add. I noticed that there was some disagreement with my statement that the danger of taking astronomy at schools was lest it should be taken as a soft option under a mistaken idea of following the line of least resistance. Personally I do not consider that it would be a soft option, and not necessarily would it be following the line of least resistance. Still, there is a possibility of that mistake being made not only in astronomy but in all sciences.

Dr. L. E. Lefevre: As far as junior work is concerned, I would like to mention what we are able to do at Eton. During one term of a boy's first year, he goes once a week to a lecture which I understand covers such topics as Mr. Edgar takes with his junior boys. I do not go to the lectures; they are given by the senior science master. As they take place at 7.30 in the morning, I have never attended! In addition, in the evenings I take boys whose interest goes beyond those lectures, and as many as the school observatory, where we have a 7-inch refractor, can accommodate—about half a dozen at a time. At the beginning of the term I sent round notices to teachers of all boys suggesting that for as many of their boys as were interested I would keep open house at the observatory each Tuesday evening for a couple of hours. The response encouraged me to believe that there was a good deal of interest among the boys, that their interest in astronomy was not merely confined to one compulsory lecture a week. On such Tuesday evenings as were fine —not very many—I have been able to stimulate their interest a good deal in the observation of such subjects as Mr. Bushell mentioned. I find that the Moon, Jupiter and Saturn, the double stars, the Sun and the more conspicuous clusters and nebulae are of considerable interest to the boys, and that it is possible to get a very real response. Boys definitely show an interest.

MATHEMATICS AT THE BRITISH ASSOCIATION, 1937.

At the meetings of the Association at Nottingham, Section A* devoted two mornings to discussions on the teaching of mathematics; on Saturday, 4th September, the subject was "The Unification of the Teaching of Algebra in Schools", and on Tuesday, 7th September, "The Influence of Higher Geometry on the Teaching in Schools". The papers read on these two occasions will be printed later as a special number of the *Gazette*. Considerable interest was shown in these discussions, Saturday's meeting being obliged to move from the room allotted to it to a larger one, and the experiment was undoubtedly successful.

The 1938 meeting will be held at Cambridge, 17-24th August.

A METHOD OF TEACHING.

By E. E. Ironmonger.

In 1816 there appeared a translation by Dr. John Taylor of a treatise on Hindu Arithmetic and Geometry, published at the expense of the Literary Society of Bombay. The author of this treatise was Bhascara Acharya (1114-*c.* 1185) and the treatise itself is of some importance in the history of Hindu mathematics. A discussion of this treatise, which is called the *Lilawati*, may be found by those interested in Rouse Ball's *History of Mathematics*, pp. 150-154. It is however with the appendix to Taylor's translation that we are here concerned. Little is known of Taylor himself except that he was born in Edinburgh, took his M.D. degree there in 1804 and died in Persia in 1821. He is believed to have published other translations from the Sanscrit besides the *Lilawati*. However, the appendix of this translation is entitled " A Short Account of the Present Mode of Teaching Arithmetic in Hindu Schools " and it may be of some interest to give a brief description of its contents.

On joining school a young pupil performed the " pati puja " or worship of the writing board. This writing board was usually an ordinary board covered with sand or brick dust or " gulal "—which is flour dyed a purple colour—and the scholar traced upon it his letters or figures with a reed or small wooden style which he was allowed to hold in whatever way was most convenient. In more advanced stages where operations became longer, they used a board painted black and wrote upon it with a mixture of chalk and water. But to return to the " pati puja " : the white surface of the board was covered with " gulal " and the form of Saraswati, the goddess of learning, was traced upon it with a wooden style. The board was then covered with perfume, rice, flowers, sugar, betel nut, etc., and near it were placed a lighted taper of incense and a burning lamp with camphor. Be it noted that all the accessories were presented to the master together with a small sum of money and a turband, or some similar present, " suitable to the condition of the parent or relation of the child." The scholar then prostrated himself before the board which was now supposed to represent the goddess Saraswati. The master wrote on the board " Shri Ganesayanama "—reverence to Ganesa, the goddess of wisdom—" Om "—the mystic name of god. A reed pen was then given to the scholar and he traced out the words on the board a few times. These preliminary ceremonies were " supposed to have a mighty influence over his future progress ".

Space will not permit us to go into the details of a scholar's curriculum here, but perhaps a brief outline of the scholar's work will give a sufficiently general idea. He began by learning the letters of the alphabet and the names and forms of the figures. Then he was taught to put down and read the figures as far as 100. This was followed by the multiplication tables which in some districts

consisted of multiplying numbers 1-10 by any number up to 30 and in others of multiplying numbers 1-10 by any number up to 100. Three tables followed in which the fractions $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$ were each multiplied by whole numbers. In the next tables these three fractions were joined to whole numbers and the numbers formed were multiplied by whole numbers, viz., one time three and a half is three and a half; three times one and a quarter are three and three-quarters, etc. Tables of money, weight, etc., were taken next together with the four elementary rules. These tables, which were all committed to memory, together with simple problems on Interest and the methods of keeping books of accounts, seem to complete the course. Thus the main bulk of the work seems to have been pure memory work on the many tables outlined above. It should however be noted that the general method of teaching was the same: the master writes down on the board the letters, tables or problems to be learnt and the scholar " retraces their forms by drawing his pen over the characters which have been written in the sand " and throughout all these operations the scholar repeated audibly " in a loud singing tone " the characters as he traced them. This did not cause confusion as might be expected (since the different scholars would be learning different tables and hence would be repeating loudly different things, etc.), but it seems to have enabled the scholar to concentrate upon his own particular work. It further enabled the teacher to observe whether anyone was idle or inattentive. " Besides ", says Dr. Taylor, " it takes away the idea of mental exertion and converts the exercises at school into a kind of play and amusement."

Before the school closed in the evening the whole class repeated the different multiplication tables: the scholars stood up and one was chosen by the master to lead, *i.e.* to say the tables so that the rest of the scholars could repeat them after him. " This proves no small incentive to each boy to make himself master of these tables, as any failure in this conspicuous position is accompanied with great disgrace." It is observed that the scholar in this way acquires a facility in performing arithmetical operations offhand, which frequently astonishes the European observer.

Dr. Taylor points to three advantages of this system of teaching: economy in writing material by the use of a sand-covered board: by oral repetition while tracing the forms of the figures or letters the separate branches of instruction, reading and writing, are thus taught together: by the use of the following kind of tables:

20 two before cipher is twenty,
21 two before one is twenty-one,
22 two before two is twenty-two, etc.,

in which the scholar writes the figures and repeats orally the words, he learns to distinguish between the nature of the relative positions of the figures and the result of adding the numbers together.

One other feature of the system is interesting when we remember

that all this is descriptive of Hindu schools of more than 130 years ago and that it is a plan of instruction by the scholars themselves. A newcomer to the school was placed under the care of one of the senior boys who had to assist the younger boy in his studies and to report upon his behaviour and progress to the master. Only a few of the elder boys received their instruction directly from the master.

"This plan of getting the older boys, and those who are more advanced, to assist those who are less advanced and younger, greatly lessens the burden imposed upon the master, whose duty, according to this system, is not to furnish instruction to each individual scholar, but to superintend the whole, and to see that every one does his duty. If the younger boy does not learn his lessons with sufficient promptitude and exactness, his instructor reports him to the master, who enquires into the case, orders the pupil to repeat before him what he has learnt and punishes him if he has been idle or negligent. As the master usually gives lessons to the elder scholars only, he has sufficient leisure to exercise a vigilant superintendence over the whole school, and by casting his eyes about continually, or walking up and down, and enquiring into the progress made by each pupil under his instruction he maintains strict discipline and keeps every one upon the alert through the expectation of being called upon to repeat his lesson." E. E. I.

1148. The main field of action of Holland's colonial activities lies in the Malay Archipelago, roughly speaking between Singapore and Australia, covering an area equal to the distance from Ireland to Iran (Persia).—"Netherlands Supplement", *Daily Telegraph*, January 4, 1937. [Per Mr. G. Braithwaite.]

1149. It is inconceivable that it could have occurred to any human being to lay down so singular a train of induction as the one above employed, unless previously prompted to do so by an *à priori* perception of the law to be established, acquired through a preliminary study and direct inspection of the earlier terms in the series of numbers to which it applies. Here then we have a salient example (if any were needed) of the importance of the part played by the *faculty of observation* in the discovery and establishment of mathematical laws.—J. J. Sylvester, *Amer. Jour. Math.*, Vol. II, p. 222 (1879), of which he was at the time Editor in Chief. [Per Dr. G. J. Lidstone.]

1150. The trading scheme has been more widely used . . . and we know that it has saved many members their subscription to the nth degree.—Report of the Executive of the Association of Scientific Workers. [Per Mr. R. O. Street.]

1151. "An umbrella, Lueli, when in use resembles the—the shell that would be formed by rotating an arc of curve about its axis of symmetry, attached to a cylinder of small radius whose axis is the same as the axis of symmetry of the generating curve of the shell. When not in use it is properly an elongated cone, but it is more usually helicodial in form."

Lueli made no answer. He lay down again, this time face downward.—Sylvia Townsend Warner, *Mr. Fortune's Maggot*, Description of an Umbrella. (Mr. Fortune, a missionary, is teaching pure mathematics to Lueli, a precocious aboriginal.) [Per Mr. E. H. Lockwood.]

TEACHING THE HISTORY OF MATHEMATICS.*

By Gino Loria.

Je viens de lire dans *The Mathematical Gazette* le savant Rapport de M. Gabriel sur l'enseignement de l'histoire des mathématiques et le compte-rendu du débat qui s'ensuivit ; combien j'ai regretté de n'avoir pas pu participer à une séance si intéressante d'une société à laquelle j'ai l'honneur d'appartenir! Dans ce cas j'aurais pu faire part à mes collègues de quelques remarques qui sont le fruit de mes expériences sur le même sujet. Voulez-vous me permettre que je vous expose en résumé ce que j'aurais pu communier aux membres de la Mathematical Association?

Je remarque avant tout que si on veut introduire des notions d'histoire des mathématiques dans l'enseignement secondaire, il faut avant tout préparer les futurs professeurs. Comme professeur universitaire j'ai cru mon devoir de faire quelques essais en ce sens, sans oublier que ce cours de préparation, n'étant pas destiné à des futurs historiens, ne devait pas avoir un caractère monographique, mais il devait embrasser la totalité des mathématiques anciennes et modernes. Depuis quelques ans j'ai fait l'histoire des mathématiques le sujet de quelques conférences détachées ; mais pendant l'année scolaire qui va maintenant finir j'ai pu faire un vrai cours d'environ 60 leçons, en profitant de certaines dispositions de nouveaux règlements universitaires italiens. C'est mon opinion qu'un cours d'histoire sur une science quelconque est utile seulement lorsque cette science est connue par ceux qui nous écoutent ; je dis cela car plusieurs traités commencent par un résumé de l'histoire de la matière ; c'est un système que je crois tout à fait stérile et que par conséquent je n'ai jamais adopté ; p. ex. rédigeant le plan de mes *Vorlesungen ueber Darstellenden Geometrie*, j'ai destiné le t. III (dernier) à l'histoire de cette science ; malheureusement, comme vous savez, la grande guerre a arrêté cet ouvrage au II volume et ma *Storia della Geometria Descrittiva* a dû paraître à part dans l'original italien.

Or, pour revenir à mon sujet, eu égard à mon cours de cette année, je pouvais m'adresser à un auditoire composé de personnes en mesure de comprendre des considérations se rapportant à toutes les branches fondamentales des mathématiques, c'est à dire algèbre complémentaire (théorie des équations), géométrie projective, géométrie descriptive, calcul infinitésimal et mécanique rationnelle. Cela m'a permis d'exposer l'histoire des mathématiques depuis les origines jusq'à la fin du XVIII siècle. C'est ce qui résulte de la table qui suit des sections de mon cours :

I. Les civilisations préhelléniques (Assyro-Babyloniens et Egyptiens). II. Origine et premier état de développement de la géométrie grecque. III. L'âge d'or de la géométrie grecque. IV. La période d'argent de la géométrie grecque. V. L'arithmétique des Grecs.

*The following article arose as a letter from Professor Loria, one of the Association's Honorary Members, to Professor Neville. The *Gazette* is fortunate in being permitted to bring before its readers the views of so well known an authority as Professor Loria on a topic of considerable interest to all teachers of mathematics.

VI. Géodésie et astronomie chez les Grecs. VII. Rome. Les siècles obscurs. VIII. Ex Oriente lux (Chinois, Indiens, Arabes). IX. Léonard Fibonacci et ses épigones immédiats en Italie. X. La Renaissance au-delà des Alpes. XI. Origine et premier développement de la perspective. XII. L. Pacioli. XIII. La période d'or de l'algèbre italienne. XIV. Progrès de l'algèbre dans les autres pays d'Europe. XV. L'humanisme dans son influence sur les progrès des mathématiques; perspective et géométrie au XVI siècle. XVI. Trigonométrie et cyclométrie au XVI siècle. XVII. Premiers ans du XVII siècle (Napier, Galilée, Kepler, etc.). XVIII. Les origines des mathématiques modernes: Descartes et Fermat. XIX. Création du calcul infinitésimal (les précurseurs, Newton, Leibniz, le célèbre débat). XX. Le XVIII siècle (mathématiciens mineurs, Euler, Lagrange, Monge). Caractères des mathématiques au XIX siècle.

J'ai fini par la remarque que le XIX siècle, dont je ne pouvais pas m'occuper car il embrasse des théories que mes auditeurs ne connaissaient pas, est caractérisé par: (*a*) la constitution de la physique mathématique, (*b*) le rôle fondamental en géométrie du concept de correspondance, (*c*) l'abandon du préjugé de la " généralité de l'analyse ", substitué par la conviction que chaque formule a un champ d'applicabilité qu'on doit déterminer dans chaque cas particulier.

L'introduction de cours universitaires sur l'histoire des mathématiques a de l'utilité de plusieurs points de vue; en effet:

1. Dans le corps des mathématiques il y a des théorèmes et des problèmes qui, quoique remarquables, ont disparus des cours ordinaires, faute de temps pour s'en occuper; le cours dont il s'agit permet de compléter de ce point de vue les connaissances des futurs professeurs.

2. L'histoire permet d'établir un pont entre les mathématiques et d'autres branches de l'enseignement, telles que l'histoire de la littérature, de l'art et de la philosophie.

3. Le cours dont il s'agit permet de ramener l'esprit des futurs professeurs secondaires sur les sujets (géométrie élémentaire, éléments de l'algèbre, trigonométrie, etc.) dont ils devront s'occuper pendant toute leur vie et qui, toutefois, sont complètement délaissés pendant toute leur vie universitaire.

J'ai reconnu le considérable intérêt que mes auditeurs avaient pour mes leçons; en effet leur nombre a été supérieur au nombre ordinaire de ceux qui suivent les cours de mathématiques supérieures; plusieurs s'adressaient au trois volumes de ma *Storia delle Matematiche* pour approfondir des sujets que je n'avais pu qu'effleurer; enfin pour la première fois on a cet an des thèses de doctorat sur des sujets d'histoire, ce qui n'avait jamais eu lieu auparavant.

Qu'il me soit permis enfin de remarquer que je me suis toujours efforcé de prouver à mes élèves que l'histoire des mathématiques est un sujet très *sérieux*, qui doit être étudié très *sérieusement*, circonstance méconnue trop souvent; je n'ai pas exclu les détails biographiques des personnes dont je parlais, mais mon attention a été toujours tourné du côté des idées, pour empêcher la reproduction des " dilettanti " qui ont jeté le discrédit sur une branche du savoir humain digne de la plus haute considération. G. L.

ON SINGULARITIES OF PLANE CURVES GIVEN BY PARAMETRIC EQUATIONS.

By C. N. Srinivasiengar.

§ 1. This note is concerned with the following statements taken from Hilton's *Plane Algebraic Curves* (chap. x), and from a note on the topic by Clifford Bell in *Math. Gazette*, Vol. XX, No. 239.

(A) If $x:y:z=f(t):\phi(t):\psi(t)$ represent a plane curve where f, ϕ, ψ are polynomials in t, the cusps of the curve are given by

$$\frac{f'(t)}{f(t)}=\frac{\phi'(t)}{\phi(t)}=\frac{\psi'(t)}{\psi(t)}, \quad \text{..........................(1)}$$

assuming that in general to each point of the curve there corresponds a single value of t.

(B) If $$F(t)=\begin{vmatrix} f & \phi & \psi \\ f' & \phi' & \psi' \\ f'' & \phi'' & \psi'' \end{vmatrix},$$

the inflexions are given by the single roots of $F(t)=0$, while the parameters of the cusps are double roots of $F(t)=0$.

These statements are correct only with regard to *simple* cusps (keratoid) and *simple* inflexions (*i.e.* inflexions whose tangents have only second-order contact). When one speaks of cusps and inflexions in connection with Plücker's equations, these simple types are usually implied, unless otherwise stated. But, in the present topic, especially in the statement (B), the results are presumably meant to be applied to any concrete example. I desire to explain that the conclusions thus arrived at may be incorrect, without further examination.

In the first place, *the equations* (1) *need not be satisfied at every cusp.** Consider the example †

$$x:y:z=(1+t)(1-2t):(1+t)^2(1-2t)^2:t.$$

The equation of the curve is $2y^2z^2+yzx^2+(yx^3-x^4)=0$. The point $(0, 0, 1)$ is a cusp, since the coefficient of the highest power of z is a perfect square. To a general point of the curve, there corresponds a single value of t, but the cusp is given by either of two values $t=-1$, $t=\frac{1}{2}$, and neither of these will satisfy equations (1). The cusp belongs to a particular type, known as the tac-node, the expansion at which for y/z in terms of x/z consists of two power-series in x/z. Hilton's result (A) is true only for those cusps which are considered to correspond to two *coincident* values of t, and the tac-node, while coming under the name "cusp", will not generally come under this criterion.

* The proof in Hilton, *l.c.* p. 138, is not convincing. It is equivalent to that of finding the conditions that $lx+my+nz=0$ meets the curve in two coincident points for all l, m, n—which appears to me to be as good as assuming the result.

† All the examples in this paper are adapted from Hilton, chapters iii and x.

§ 2. Let $lx+my+nz=0$ be a tangent having contact of order r at an ordinary point of the curve. Then

$$\begin{vmatrix} f & f' & f'' & \dots & f^{(r)} \\ \phi & \phi' & \phi'' & \dots & \phi^{(r)} \\ \psi & \psi' & \psi'' & \dots & \psi^{(r)} \end{vmatrix}$$

is a matrix of rank two. Hence,

$$F(t)=0,\ F'(t)=0,\ \dots\ F^{(r-2)}(t)=0,$$

i.e. t is an $(r-1)$-ple root of $F(t)=0$. Expressed in words, a hyper-inflexional point, the tangent at which has contact of order r with the curve, is equivalent to $r-1$ simple inflexions (compare Hilton, p. 102, exs. 4 and 5).

Consider next a cusp which corresponds to two coincident values of the parameter. The equations (1) are satisfied, and the equation of the cuspidal tangent is

$$\begin{vmatrix} x & y & z \\ f & \phi & \psi \\ f'' & \phi'' & \psi'' \end{vmatrix}=0,$$

provided that $f'':\phi'':\psi''$ are not equal to $f:\phi:\psi$—which case would refer to a singularity higher than a double point.

Let us suppose that the cuspidal tangent meets the curve $s+3$ times at the cusp. Then, if $lx+my+nz=0$ be the tangent,

$$lf^{(p)}+m\phi^{(p)}+n\psi^{(p)}=0,\ (p=1, 2, \dots, s+2).$$

Therefore,

$$\begin{vmatrix} f & f'' & f''' & \dots & f^{(s+2)} \\ \phi & \phi'' & \phi''' & \dots & \phi^{(s+2)} \\ \psi & \psi'' & \psi''' & \dots & \psi^{(s+2)} \end{vmatrix} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots(2)$$

is a matrix of rank two, t being the value of the parameter at the cusp.

Now, $F(t)=\text{determinant }(f, \phi', \psi'')$; $F'(t)=(f, \phi', \psi''')$;

$$F''(t)=(f, \phi'', \psi''')+(f, \phi', \psi^{\text{IV}}).$$

Hence, by (1) and (2), $F''(t)=0$, if $s=1$, *i.e. at a ramphoid cusp*. Similarly $F'''(t)=0$, if $s=2$, and so on.*

In any concrete example, therefore, the double roots of $F(t)$ may correspond to cusps, or to points of undulation. Similarly $F(t)=0$ may admit of higher multiple roots which may correspond either to higher cusps or to higher inflexions.

Examples.

(i) What is the nature of the origin for the curve

$$x:y:z=(t+1)^6:t(t+1)^4:t^4\ ?$$

If $t=-1$, we find $f:\phi:\psi=f^{(r)}:\phi^{(r)}:\psi^{(r)}$, $(r=1, 2, 3)$. The deter-

* These are the lowest derivatives of $F(t)$ which vanish for a given value of s; it is easy to see that some higher derivatives of $F(t)$ may also vanish. See example (iii), in which $s=4$ when $t^{-1}=u=0$, but the first seven derivatives of $F(u)$ vanish.

minant $(f, \phi''', \psi^{\mathrm{IV}})=0$ reduces to $x=0$. Hence the point $(0, 0, 1)$ is a quartuple point with four coincident tangents coinciding with $x=0$.

(ii) The curve $x:y:z=1-2t^2:t(1-3t^2):t^4$. Here

$$F(t)=12t^2(1-t^2)^2.$$

While $t=\pm 1$ give simple cusps, $t=0$ gives a point of undulation. $t=0$ does not satisfy (1), but makes $(f, \phi'', \psi''')=0$.

(iii) $x:y:z=t(2-t):t^4(2-t)^3:1$. Here t occurs as a squared factor in $F(t)$ while $t-2$ occurs as a simple factor. $t=0$ and $t=2$ both give the point $(0, 0, 1)$. Hence, we have at this point one linear branch with a point of undulation, and a second linear branch with a point of inflexion. The tangents happen to be the same, viz. $y=0$. The point is a cusp (a specialised tac-node), as is evident from the equation of the curve in the form $y^2z^6-2yz^3x^3+x^7=0$. The equations (1) are not satisfied by either value of t.

Changing t to u^{-1}, we get $x:y:z=u^5(2u-1):(2u-1)^3:u^7$. The value $u=0$ satisfies (1) and also (2), for the value $s=4$. Hence there is a cusp at infinity, the cuspidal tangent meeting the curve seven times at the cusp. $F(u)$ contains u^8 as a factor.

(iv) The curve $x:y:z=f(t):\phi(t):\psi(t)$ where f, ϕ, ψ are polynomials of degree n with the coefficients of t^{n-1} and t^{n-3} zero has a ramphoid cusp.

By changing t to u^{-1}, and by a change of origin, we can write

$$x:y:z=a_1u^2+a_2u^4+\ldots:b_1u^2+b_2u^4+\ldots:c_0+c_1u^2+c_2u^4+\ldots .$$

The determinant (f, ϕ'', ψ''') vanishes for $u=0$. Hence $u=0$, which also makes $f'=\phi'=\psi'=0$, gives a cusp whose tangent meets the curve four times at the cusp. C. N. S.

1152. At the siege of Syracuse he [Archimedes] kept the Roman army in check for three years with his war engines and burning glasses, and, if we may believe the story, he had just discovered how to square the circle when a soldier cut him down as he bent over his diagram.—F. A. Wright, *The Romance of Life in the Ancient World*, p. 184. [Per Mr. W. A. Garstin.]

1153. From a script.

To prove that the base angles of an isosceles triangle are equal. In the triangles ABC, ACB,

$$AB=AC,$$
$$AC=AB,$$
$$\angle BAC=\angle CAB.$$

Therefore the triangles are congruent and $\angle ABC=\angle ACB$.

1154. Of the date of this origin, however, I grieve that I can only speak with that species of indefinite definiteness which mathematicians are, at times, forced to put up with in certain algebraic formulae. The date, I may thus say, in regard to the remoteness of its antiquity, cannot be less than any assignable quantity whatsoever.—E. A. Poe, *The Devil in the Belfry*. [Per Mr. B. A. Swinden.]

CORRESPONDENCE.

INDIRECT PROOF IN GEOMETRY.

Dear Sir,—I should like to make some observations on the points raised by Mr. Leadbeater in his article on "Indirect Proof in Geometry" in the February number of the *Gazette* (p. 25). He charges writers of school geometries with lack of logic because they "falsify the data" in their figures: for example, in proving that a quadrilateral with supplementary opposite angles is cyclic, they draw figures in which the opposite angles are not supplementary. Does a figure which is inconsistent with the data invalidate the proof? How far is a figure a constituent of the proof? Most of us have been confronted with this question in the class-room. We announce our intention of proving that the diagonals of a parallelogram bisect each other, perhaps we carelessly draw a figure which is a bad attempt at a parallelogram, and then say: "to prove that its diagonals bisect each other". "But", says a member of the class, "your figure is not a parallelogram," to which the reply is given, "Perhaps not, but *if* it is a parallelogram, its diagonals will bisect each other, and *that* is all I am going to prove." I think Mr. Leadbeater would admit that in this case the reply is satisfactory. Would the proof of the theorem of Pythagoras be invalidated, if the printed figure were badly inked so that one of the "squares" had a missing corner? And anyway no theorem refers to the figure actually printed only, but to all of a certain type. How then can the proofs in question fail, merely because the angles in a printed figure are, or appear to be, non-supplementary.

If I were out only to score debating points, or to demolish a critic, I would leave it at that, but I think that the objection merits a little more notice, for after all, in school geometry, we *do* make inferences from figures. Consider two extreme standpoints; (1) we may treat geometry entirely abstractly; it is then not concerned with empirical space, but with any entities satisfying certain postulates. Figures are then certainly not constituents of the proof, they are only aids to understanding, necessary because our books are written not for angels, but for fellow-worms; (2) we might treat geometry entirely empirically, and assert that the medians of a triangle concur because in a carefully drawn figure they appear to do so. Geometry is then an empirical science like experimental electro-magnetism. In school geometry we take a course between these two; we begin with the second point of view, appealing, if not to measurement, at least to spatial intuition, and we move towards, but never reach, the first point of view. Consider our parallelogram question again; the sides of the "parallelogram" we draw cannot be absolutely parallel or quite straight or devoid of thickness, but this does not invalidate our proof; but we must not draw a re-entrant quadrilateral, because then the usual school proof would fail, and for this reason: the usual proof is incomplete, inasmuch as it assumes

that a diagonal of a parallelogram lies inside it. This is read from the figure, while the rest of the argument does not depend on the figure.

How much then may be read from the figure in school geometry? Tradition allows us to assume as evident, and without fuss, all theorems on order-relations (between, inside, and so forth), if they are " obvious " from the figure, but not, for example, relations of magnitude, such as equality of lengths or angles. Thus in his very first proposition, Euclid assumes, without saying so, that a circle, which goes through a point inside and a point outside another circle, cuts the latter circle. This is read from the figure, and the figure must be drawn so as to give it.

Now the properties of the figure to which Mr. Leadbeater takes exception are those which, by common consent, are not constituents of the proof. The school treatment of geometry is essentially a compromise between the abstract and the empirical treatments; reasoning on figures inaccurately drawn helps the pupil along the path that leads to the abstract method, provided these figures do not appear too early in the course, and the only crime that I and my companions in the dock have committed is that of adhering to the traditional compromise. What else can one do? The abstract method is impracticable, the empirical method useless.

H. G. Forder.

University College, Auckland, New Zealand.

" A WEIGHTY MATTER."

Dear Sir,—It is so difficult for even the most careful teacher to use the terms *mass* and *weight* without talking nonsense or shall we say " using phraseology open to damaging criticism ", that I never start to read an article, even in the *Mathematical Gazette*, which uses these terms without an expectation of finding fallacious arguments in it.

Consider Mr. Grattan-Guinness' introduction of the critical idea " mass ", p. 143 (2.2). " The two objects have been made identical as far as we can see; they must therefore have equal masses (whatever we mean by that) for being identically equal they are equal in *every* respect. Thus equal masses will balance. . . ."

In the first of these sentences " mass " is a *property* of the object; in the second it has become the object itself. The fallacy will perhaps become evident if for " mass " we substitute in the argument some other property of the object, thus:

" The two objects have been made identical; they must therefore have equal smells (whatever we mean by that) for being identically equal they are equal in *every* respect. Thus equal smells will balance . . . ."

Not only is the argument fallacious but the result is unsatisfactory. We must not confuse mass which is only a property of the object, even though it may be the most important and fundamental one,

with the object itself. If we do, what is to happen when following Einstein we move the object so rapidly that its mass changes ?

Again, p. 143 (3.3), " weight is not measured on a balance ". I have heard more than one person, to the eminence of whose knowledge I look up, assert that what a balance tells is " mass ", but am not quite convinced. In these days of accurate measurement to I do not know how many decimal places, is it too fanciful to suppose the arms of a balance lengthened until the value of g at one end could be distinguished from that at the other? If this could be done, would equal masses still balance or should we have mg equal to $m'g'$?

Possibly I have gone far enough to convince Mr. Grattan-Guinness that there are more pitfalls than he had supposed. Anyhow let me make a positive suggestion, instead of mere negative criticism, and so give him or another the chance of pointing out that I am writing nonsense.

I am not concerned with Chemistry, etc., but in elementary Mechanics the only definition or explanation which we can give of " mass " (suggested by the phrase " inverse ratio of accelerations ") is one in which the word " inertia " can replace the word " mass ". If therefore we are using the word " mass " in its accurate sense it can be replaced by the word " inertia ". If the word " mass " cannot be replaced, in elementary Mechanics, by the word " inertia " then the word " mass " is not being used in its accurate sense but in one of its popular senses such as might be employed by " the butcher, the baker or even perhaps the candlestick-maker ".

It is possible that a book on mechanics may some day appear in which all words are used in their precise mathematical senses. The best chance of this might perhaps be to drop the word " mass " throughout and replace it by " inertia ", but it is more likely that the precise author would keep the various properties of his objects distinct by speaking of " the weight of 1 lb.", " the mass of 1 lb.", or by using " pound-weight " and " pound-mass ". Whether the boy would learn more quickly and accurately from such a book is doubtful.

Anyhow I contend that the Mathematical Association was prudent in permitting us still to talk about " winding up the weights of a clock " and to use other such popular phrases. To try to force everyone to refrain from these would be to encounter a mass of inertia difficult to overcome. True there may be a mass of arguments in favour of Mr. Grattan-Guinness' suggestions but at the date of the *Report on the Teaching of Mechanics* the weight of considered opinion was against him.

C. O. Tuckey.

Dear Sir,—Mr. Grattan-Guinness has pointed out very well the need for a clear understanding and statement of units in the early treatment of mechanics. But in the treatment he advocates for the early presentation, to children of 12 upwards, his definition of

mass seems to me wrong and misleading. He suggested that the measurement and first ideas of mass should be obtained by balancing on an ordinary pair of scales. Now the scale balance is a simple example of a lever, and a lever is used to give a first introduction of "force". Thus when a load is hung from a lever, it is customary to recommend the beginner to draw a diagram of the lever with an arrow head to indicate the magnitude and direction of the force which is the weight of the load. It is then possible to point out that this force could be produced in other ways, and hence the measurement of force in lb.-wt., *i.e.* the weight of the numbers of lbs. in the load, is explained. If this treatment is followed then the definition of mass given by Mr. Grattan-Guinness would lead the boy to talk of the force due to the mass of the load and would give him an idea of mass that has later to be undone. I consider that a scale pan compares the weights of two bodies, and only when a boy knows the relation between mass and weight should he realise that the scale pan also compares masses.

Mass is a concept which requires gradual introduction and a late definition (6.4 and 6.5, pp. 25-28 of the *Mechanics Report*) suggests a method by which this can be done. I would add a few remarks to this. As soon as the expressions for momentum and energy are obtained—in gravitational units—it should be explained that each of these quantities depends on the velocity of the body and its size or mass. Boys should by this stage be quite clear that weight is a force, but that the W in these expressions is really a measure of something more permanent and constant, inherent in the body, which can be called mass. If some boy is sufficiently clear-headed to protest that W is not constant, the retort should be that W/g is constant. The presence of the g should at any rate be explained as necessary for the particular units, ft. lb. wt. and sec. lb. wt., and the statement can be made that the ultimate measure of momentum, for example, is mass times velocity, when appropriate units have been introduced. Thus a boy should be thoroughly familiar with the word *mass*, which should be used as often as possible, and with the ideas involved, before he meets the definition in the fundamental equation $P=mf$.

The M.A. Report does, in respect of mass, give the lead for which Mr. Grattan-Guinness is asking, even if an alternative treatment is also given. In respect of units the Report does appear to compromise, and the article has shown very clearly why it is essential, in the early stages, that boys should be clear about the units involved and should state them.

K. S. Snell.

GLEANINGS: AN APPEAL.

The Editor will be grateful for help in the filling up of odd corners. A precise reference should accompany every quotation.

MATHEMATICAL NOTES.

1247. *A proof of Euclid XI, 4.*

AO, OB are two lines to both of which CO is perpendicular. We have to show that CO is perpendicular to any line OX in the plane AOB. OL, OM are the two bisectors of the angles between OA and OB, and for clearness are shown in separate figures.

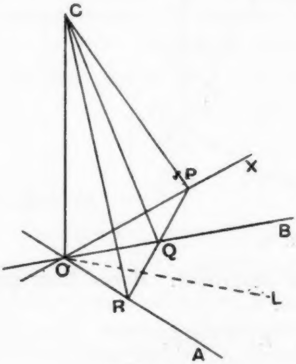


FIG. 1.

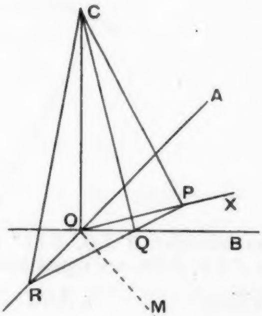


FIG. 2.

If CO is not perpendicular to OX, draw CP perpendicular to OX and through P draw a line equally inclined to OB and OA to cut them in Q and R respectively so that Q and R are on the same side of P, that is, PQR is perpendicular to the bisectors OL, OM.

Then triangle QOR is isosceles, and so the triangles COR and COQ are congruent. Hence the triangle CRQ is isosceles.

Thus $\angle CQP > 90°$,

and hence $CP > CQ$.

But since $\angle COQ = 90°$, $CQ > CO$.

Hence $CP > CO$.

But, by hypothesis, $\angle CPO = 90°$, and so

$$CP < CO.$$

We have a contradiction; hence CO is perpendicular to OX.

W. J. THOMPSON.

1248. *To draw the square of which a given parallelogram is the orthogonal projection.*

A solution of this problem was given by Mr. E. H. Smart in Note 940, *Gazette*, XIV, p. 462. The method was an elegant use of the properties of conjugate diameters of an ellipse.

It is easy to see that the problem is equivalent to another: "Draw a line through the vertex of a triangle so that the perpen-

diculars on it from the extremities of the base form with the sides triangles of equal area."

For, suppose $ABCD$ is a parallelogram which is the projection of a square $A'BC'D'$ and $A'X$, $C'Y$ are perpendiculars on the line of intersection XBY of the two planes.

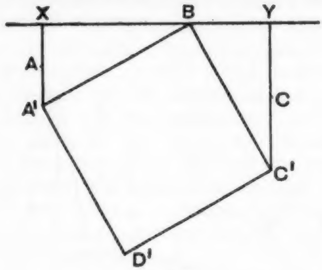


FIG. 1.

The triangles $A'XB$, BYC' are congruent. Hence their projections AXB, BYC are equal in *area*.

Writing $\angle ABX=\theta,\quad \angle CBY=\phi,$

$$AB=c,\qquad BC=a,$$

we have $\theta+\phi=\pi-B$ and $c^2 \sin 2\theta=a^2 \sin 2\phi$.

The circumcircle of the triangle ABC is convenient for solving these equations.

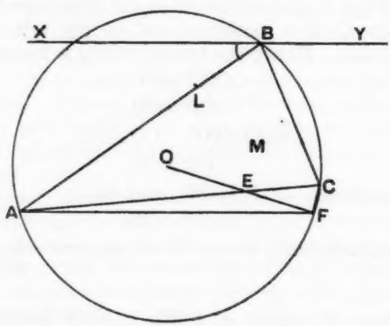


FIG. 2.

Let CL be parallel to the tangent at B, M the mid-point of CL. The line BM meets AC in a point E such that

$$AE : EC=AB^2 : BC^2.$$

The radius OE produced to cut the arc AC opposite to B in F

determines the angles AFE, CFE which are ϕ, θ. The proof consists in showing that

$$\frac{AB^2}{BC^2}=\frac{AE}{EC}=\frac{\sin AOE}{\sin COE}=\frac{\sin 2\phi}{\sin 2\theta}.$$

B. E. LAWRENCE.

1249. *The biquadratic equation.*

Whatever method by formula is used to solve

$$x^4+2B_1x^3+B_2x^2+2B_3x+B_4=0,$$

we must first solve the three-termed cubic

$$\begin{vmatrix} 2B_2-z, & 3B_1\ , & 6B_3 \\ 3B_1\ , & 3\ , & z+B_2 \\ 6B_3\ , & z+B_2, & 12B_4 \end{vmatrix}=0,$$

then calculate

$$W=\tfrac{1}{6}(z+B_2),\quad a_1=\surd(2W+B_1^2-B_2),\quad a_2=\surd(W^2-B_4),$$

choosing signs so as to satisfy $a_1a_2=B_1W-B_3$.

Finally solve $\quad x^2+B_1x+W\pm(a_1x+a_2)=0.$

This is the outcome of the method of Ferrari, and is somewhat more direct than that of Lagrange.

The method of Descartes amounts to this: get

$$C=B_1^3-B_1B_2+2B_3$$

and l and m from $\frac{1}{2}(m+l)=4W-B_1^2$, $\frac{1}{2}(m-l)=2C/a_1$; then solve

$$x^2+(B_1+a_1)x+\tfrac{1}{4}(B_1^2+2B_1a_1+l)=0,$$
$$x^2+(B_1-a_1)x+\tfrac{1}{4}(B_1^2-2B_1a_1+m)=0.$$

Euler's method, applicable only in the irreducible case of the cubic, is in short: suppose f, g, h are the numerical values of a_1 corresponding to the three values of z. Get

$$y_1=-f+g+h,\quad y_2=f-g+h,\quad y_3=f+g-h,\quad y_4=-f-g-h$$

when C is positive, but change signs when C is negative. These four results will be the roots of the equation obtained by putting

$$x=\tfrac{1}{2}(y-B_1).$$

H. ORFEUR.

1250. *On relative velocity.*

In looking over some back numbers of the *Gazette*, my attention was called to a Note on Relative Velocity by D. A. Young (No. 1046, October 1932). This note was largely concerned with notation; and as I have been using for years a notation similar to Mr. Young's, but with certain modifications that seem to help pupils over their difficulties, it occurred to me that a description of this might interest my fellow-teachers.

The velocity of A relative to B is denoted by V_{AB}. All velocities are treated as relative, and in problems where the earth is the frame

of reference a phrase like "the actual velocity" is immediately translated into "the velocity relative to the earth". Methods of defining relative velocity and establishing the relation between different velocities will vary considerably, but the fundamental result, however approached, must boil down to

$$V_{AB} + V_{BC} = V_{AC},$$

the addition being, of course, a vector addition.

In constructing the vector diagram necessary for the solution of a problem, the vector which represents V_{AB} is itself denoted by AB. This device reduces most ordinary problems on relative velocity to simple questions of constructing triangles (or quadrilaterals) to conform to given data. (That is all the problems amount to in any case, but this method helps the beginner to understand what sort of diagram he must draw.) The method can be made clear by a few examples. For this purpose I have chosen the two given by Mr. Young, so that I may be free from the suspicion of special pleading, and have added three others of rather greater complexity chosen from a standard textbook.

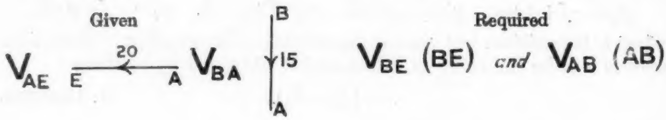
(1) To an observer on a ship which is travelling W. with a speed of 20 M.P.H. a second ship appears to be moving S. with a speed of 15 M.P.H. What is the actual speed and direction in which the second ship is moving? In what direction and with what speed would the first ship appear to move to an observer on the second?

(We denote the ships by any convenient letters, say A and B, and the earth by E; for convenience, the information given and required is summarised in the scheme below.)

We have to construct a triangle ABE in which AB and AE have the magnitudes and directions shown below.

Objects

First ship (A). Second ship (B). Earth (E).



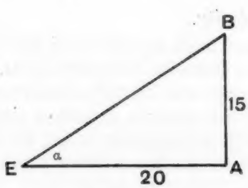
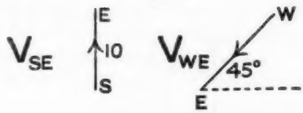


Fig. 1.

BE and α are easily calculated or measured.

(2) A steamship is travelling north at the rate of 10 M.P.H. and there is a N.E. wind blowing at the rate of 20 M.P.H. In what direction will the smoke from the funnel appear to move to an observer on the ship?

Objects	Given	Required
Steamer (S)		Direction of WS
Wind (W)		(V_{WS})
Earth (E)		



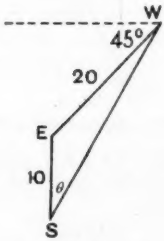


FIG. 2.

θ is easily calculated.

(3) Find the direction of the trail of smoke of a steamer moving N. at 10 k. relatively to the water in a current which sets N.W. at 2 k., the wind blowing from the W. at 5 k.

Objects	Given	Required
Steamer (S)		Direction of
Water (W)		AS
Air (A)		
Earth (E)		

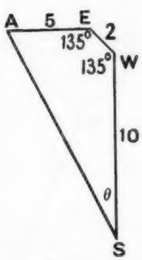
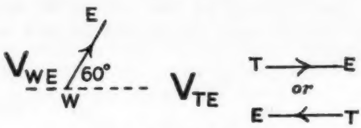


FIG. 3.

θ is easily calculated.

(4) A wind is blowing at 20 M.P.H. in a direction making an angle of 60° with a road. Find the speed of a traveller along the road when the speed of the wind appears to be least.

Objects	Given	Required
Wind (W) Traveller (T) Earth (E)		Magnitude of TE when WT is least

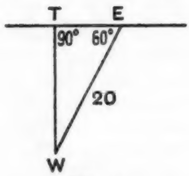


FIG. 4.

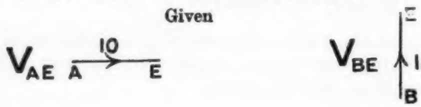
$$ET = 20 \cos 60^\circ = 10.$$

(5) To a cyclist riding E. at 10 M.P.H. a wind which is S.W. appears to be S.E. Find the apparent direction of the wind to a second cyclist riding N. at 10 M.P.H.

Objects

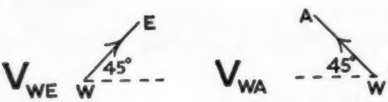
First cyclist (A). Second cyclist (B). Wind (W). Earth (E).

Given



Required

Direction of WB



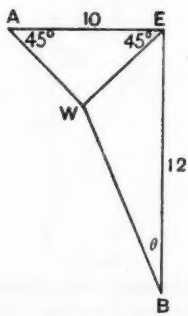


FIG. 5.

θ is easily calculated.

J. H. DOUGHTY.

1251. *Note on division.*

In his article "Method of Long Division for Small Divisors", (Vol. XX, p. 331), Mr. Fletcher-Jones uses a method which I always thought was a common device among those of us whose unwelcome task it is to have to percentage form marks once a fortnight! However, since reading his paper, I have made enquiries in my school and have found but one member of the Staff and one of the VI Form familiar with the process.

Personally I think the method is best illustrated thus:

To divide 674853 by 59 (59 is a nasty number, it is so near 60 that we find ourselves wishing it were 60). Well, let us replace 59 by 60 and apply a correction at each step of the division:

$$59\ (60)\ \underline{)\,674853}$$
$$11438+11$$

60 divides into 67 once with remainder 7, hence 59 divides once with remainder $(7+1)$ or 8. 60 into 84 goes once with remainder 24, hence 59 divides once with remainder $(24+1)$ or 25. 60 into 258 divides 4 times with remainder 18, hence 59 divides 4 times with remainder $(18+4)$ or 22. 60 into 225 goes 3 times with 45 over, hence 59 divides 3 times with $(45+3)$ or 48 over. 60 divides into 483 8 times with 3 over, hence 59 goes 8 times with $(3+8)$ or 11 over. Once it is understood that the correction to be applied to each remainder is equal to the quotient in that step the process is easy. The explanation is simple, 59 is 1 less than 60! Of course the next step is to save time by dividing by 6 instead of 60.

Had the divisor been 58 we could proceed as before and divide by 60, but clearly the correction to be applied after each step will now be twice the quotient in that step.

The method will apply to 61, but this time we will have to subtract the correction after each step. For example, to divide 674853 by 61:

$$61\ (60)\ \underline{)\,674853}$$
$$11063+10$$

60 divides into 67 once with remainder 7, hence 61 divides once with remainder $(7-1)$ or 6. 60 divides 64 once with remainder 4, hence 61 divides once with remainder $(4-1)$ or 3. 60 will not divide 38. 60 divides into 385 six times with remainder 25, hence 61 divides six times with remainder $(25-6)$ or 19. 60 divides 193 three times with remainder 13, hence 61 divides three times with remainder $(13-3)$ or 10.

After a little practice one can readily apply the method to such divisors as 249, 251, 298, 301, etc. J. C.

1252. *Note on the teaching of Fletcher-Jones division.*

The following explanation of the method of division given in the December *Gazette* may be of use and interest to teachers. It enabled all my top form to grasp the method and its justification at once.

Dividing by 29 is finding out how many groups of 29 one can get

out of a larger number. This explanation regards the method as a division by 30 with a correction or adjustment at each step. Further, it enables one to extend the method to a division by 31 without great difficulty.

I first showed the class 3 small boxes made of such a size that they exactly held 10 small flat bricks each, and pointed out that if we wanted to take out groups of 29, but actually took out all these, then one odd brick would have to be returned for division in the next column for each group of 30 taken. I referred back to the boxes and bricks from time to time for emphasis.

To take an actual example given by Mr. Fletcher-Jones :

$$\begin{array}{r} 3\,)\,1\;2\;3\;4\;5 \\ {\scriptstyle 7\;16} \\ 4\;2\;5 \end{array}$$

First 3 into 12 goes 4 and this is written one column on, as one is really dividing by 30.

Now consider what is the next number to be divided : there is the 3 of the given sum, together with a correction due to the fact that one took out 4 groups of 30 whereas one should have taken only groups of 29. This correction is clearly equal to the quotient itself, thus giving $3+4$, or 7.

At the next step one has a remainder of 1 (which of course counts as 10 in the next column) plus 4 from the given sum and a further 2 due to one having, at the last step, taken 2 groups of 30 instead of 29 . . . . Though with the class I continued the explanation to the end of the sum, it seems unnecessary to do so here.

Naturally this is only the outline of the explanation but it seems to be suitable for consideration by quite average boys.

To divide by 31, all one has to do is to employ the correction the other way on, *i.e.* the quotient is subtracted each time instead of added, with the result, however, that it is often necessary to go back and make readjustments similar to those required in finding square roots by the long division method. We tried it out and found it quite satisfactory.

In dividing by a number such as 28, the correction is clearly 2 for each group of 30 one takes out, *i.e.* one has to add in double the quotient each time. C. D. LANGFORD.

1253. *A method of division.*

In the December number of the *Mathematical Gazette,* I notice the article by A. A. Fletcher Jones, " Method of Division for Small Divisors ", pp. 331-2. Inasmuch as the author asks whether this method has been discovered before, I may say that methods which are essentially the same in principle have been published many times. I give below some of the references which I have at hand, and undoubtedly the list can be extended.

Mathematical Gazette, Vol. I. Edward M. Langley. " Some Curiosities in Division ", pp. 205-8 ; 275-6.

Fourier seems to be the original author. See Fourier, *Analyse des équations déterminées*, p. 187.

A. De Morgan. *A Budget of Paradoxes* (Longmans, 1872, p. 292; (2nd edition) Open Court Publishing Co., Chicago). The method is here ascribed to Geo. Suffolk, who published a book on his "Synthetic Division". See also Johnson's *Cyclopedia* (New York; 1st edition about 1876), article "Synthetic Division". Not given in the last edition.

Mathematical Magazine, Vol. I, p. 7 (Washington, D. C.). Gives the method applied to finding $\frac{1}{19}$ as a repeating decimal.

Edouard Lucas. *L'Arithmétique Amusante* (1895), pp. 81-2. Gives division by 19, 199, 1999, ... ; also by 29, 299, 2999, ...

Edward M. Langley. *Computations*, "Approximations in Division", pp. 71-2. Shows how to reduce $\frac{1}{98}$, $\frac{1}{998}$, $\frac{1}{9997}$, $\frac{1}{97}$, $\frac{1}{997}$, $\frac{1}{9899}$ to decimals.

William F. White. *A Scrap-Book of Elementary Mathematics* (Open Court Pub. Co., Chicago), pp. 23-4, 40-41.

Article "Corrective Division" in Van Nostrand's *Eclectic Engineering Magazine*, Vol. 29.

J. Lüroth. *Numerische Rechnen* (1900), p. 38.

R. Mehmke. "Numerisches Rechnen" (*Encyk. der Math. Wiss.*, I, pp. 940-4) gives many references to similar methods. The Austrian (or Italian) method of division is similar.

C. S. Peirce. "A New Rule for Division in Arithmetic" (*Science*, Vol. 2 (1883), 788-9) seems to be similar.

The method may be extended not only to numbers ending in 9 or 1, but to factors of these numbers.

Thus, to divide by 13, multiply dividend by 3 and divide by 39. Other numbers which may be reduced to convenient form are

$$
\begin{gathered}
13 \times 7 = 91, \\
17 \times 3 = 51, \\
17 \times 7 = 119, \\
23 \times 3 = 69, \\
23 \times 13 = 299, \\
37 \times 3 = 111, \\
43 \times 7 = 301, \\
47 \times 17 = 799, \\
53 \times 17 = 901, \\
67 \times 3 = 201, \\
73 \times 137 = 10\ 001, \\
73 \times 1\ 369\ 863 = 99\ 999\ 999 \\
83 \times 13\ 253 = 1\ 099\ 999, \\
97 \times 6\ 185\ 567 = 599\ 999\ 999, \\
103 \times 7767 = 800\ 001, \\
107 \times 3\ 738\ 317\ 757 = 399\ 999\ 999\ 999.
\end{gathered}
$$

To divide 1 by 251. If we have the result ·00398 40637 ... we can check the answer by multiplying it by 251, *i.e.* adding to the result 25 times itself moved to the left one figure or divided by 4 and moved 3 figures to the left.

$$\begin{array}{rr} & \cdot 00398\ 40637 \\ & \underline{\cdot 99601\ 59362} \\ \text{Adding} & \cdot 99999\ 99999 \end{array}$$

Since the sum is 1 (or ·99999 ...) the second number is the complement of the first. The first figure of the quotient ·003 is found by inspection and the complement ·996 written below it. Multiplying this complement by 4 and moving the product 3 figures to the right we have the quotient to 6 figures ·00398 4, writing the corrected complement below ·99601 5 and multiplying by 4 we get the next 3 figures. On account of not keeping track of the unknown figures in the complement, the quotient figures will often have to be corrected, but usually only the last 2.

This method is very rapid. To divide other numbers beside 1 by 251, the dividend is written in place of the sum ·99999 ... .

For example,

$$\begin{array}{rl} 251\ ) & \underline{346{\cdot}28} \\ & 1{\cdot}3796015936 \\ & \underline{344{\cdot}9003984064} \\ & 346{\cdot}28 \end{array}$$

In a similar way we can divide by 2501, 25001, ...

To divide by 249. Since $249 = 250 - 1$ and the first significant digit is found by inspection to be 4, the next three digits of the quotient are found by multiplying the first 3 by 4, *i.e.* ·004016; the next 3 digits by multiplying this by 4, *i.e.* ·004016064. If a check is desired, the complement may be written below and the same rule holds for the complement as for the quotient:

$$\begin{array}{rl} 249\ ) & \underline{1{\cdot}00000\qquad\quad} \\ & \cdot 004016064256 \\ & \cdot 995983935743 \end{array}$$

The same method may be used to divide by 2499, 24999, ...

Since $\frac{1}{83} = \frac{3}{249}$, we can divide by 83 by first multiplying by 3:

$$\begin{array}{rl} 83\ ) & \underline{1{\cdot}00000\ 00000} \\ & \cdot 012048192768 \end{array}$$

To divide by 1251. Similar to division by 251, except that the complement is multiplied by 8, giving the next four figures of the quotient.

Similarly to divide by 12501, 125001, ... ; 1249, 12499, 124999, ...

This method is one of Iteration, so the result checks itself.

E. B. Escott.

1254. *Comments on Note* 1225.

In Note 1225, E. H. N. incidentally raised the question why the authors of *Modern Analysis* described Bromwich's inequality

(1) $$0 \leqslant e^{-t} - \left(1 - \frac{t}{n}\right)^n \leqslant \frac{t^2}{2n}, \quad (0 \leqslant t \leqslant n)$$

(with n a positive integer) as " less precise " than the inequality

(2) $$0 \leqslant e^{-t} - \left(1 - \frac{t}{n}\right)^n \leqslant \frac{t^2 e^{-t}}{n}, \quad (0 \leqslant t \leqslant n)$$

seeing that $t^2 e^{-t}/n$ exceeds $t^2/(2n)$ in part of the range $0 \leqslant t \leqslant n$. I hope that I am right in assuming that the motive which actuated E. H. N. in raising this question was anxiety lest any of his friends should incur the judgment described in Matthew xii, 36.

I cannot imagine anyone regarding the inequalities as being of any intrinsic interest ; the only object in constructing them is to establish the equivalence of the two definitions of the Gamma-function by proving that

(3) $$\lim_{n \to \infty} \int_0^n t^{x-1} \left(1 - \frac{t}{n}\right)^n dt = \int_0^\infty t^{x-1} e^{-t}\, dt,$$

it being easy to show by partial integrations that the integral on the left is equal to

$$\frac{n!\, n^x}{x(x+1) \ldots (x+n)}.$$

Consider in turn the results of using (2) and (1) to attain this end.

(A) To derive (3) from (2), with x real and positive, it is sufficient to observe that

$$\begin{aligned} 0 &< \int_0^n t^{x-1} \left\{ e^{-t} - \left(1 - \frac{t}{n}\right)^n \right\} dt + \int_n^\infty t^{x-1} e^{-t}\, dt \\ &< \frac{1}{n} \int_0^n t^{x+1} e^{-t}\, dt + \int_n^\infty t^{x-1} e^{-t}\, dt \\ &\to 0, \end{aligned}$$

as $n \to \infty$, since both of the integrals $\int_0^\infty t^{x \pm 1} e^{-t}\, dt$ are convergent.

(B) On the other hand, the reasoning used in (A) seems inadequate to derive (3) from (1). For this purpose, one may either make an appeal to a theorem of Tannery's type (*i.e.* a theorem which really concerns *double* limits ; its supposed difficulty was swept away by E. H. N. seven years ago in Note 963 in the *Gazette*) ; or one may give the reasoning in full by first choosing an arbitrary positive number X, and then, for $n \geqslant X$, it is clear that

$$\begin{aligned} 0 &< \int_0^X t^{x-1} \left\{ e^{-t} - \left(1 - \frac{t}{n}\right)^n \right\} dt + \int_X^n t^{x-1} \left\{ e^{-t} - \left(1 - \frac{t}{n}\right)^n \right\} dt + \int_n^\infty t^{x-1} e^{-t}\, dt \\ &< \frac{1}{2n} \int_0^X t^{x+1}\, dt + \int_X^n t^{x-1} e^{-t}\, dt + \int_n^\infty t^{x-1} e^{-t}\, dt, \end{aligned}$$

whence

$$\begin{aligned}0 &\leqslant \varliminf_{n\to\infty}\left[\int_0^\infty t^{x-1}e^{-t}\,dt - \int_0^n t^{x-1}\left(1-\frac{t}{n}\right)^n dt\right]\\ &\leqslant \varlimsup_{n\to\infty}\left[\int_0^\infty t^{x-1}e^{-t}\,dt - \int_0^n t^{x-1}\left(1-\frac{t}{n}\right)^n dt\right]\\ &\leqslant \varlimsup_{n\to\infty}\left[\frac{1}{2n}\int_0^X t^{x+1}\,dt + \int_X^\infty t^{-1}e^{x-t}\,dt\right]\\ &= \int_X^\infty t^{x-1}e^{-t}\,dt.\end{aligned}$$

Since $\int^\infty t^{x-1}e^{-t}\,dt$ is convergent, this shows that

$$\varlimsup_{n\to\infty}\left[\int_0^\infty t^{x-1}e^{-t}\,dt - \int_0^n t^{x-1}\left(1-\frac{t}{n}\right)^n dt\right]$$

cannot have any value greater than zero ; and from

$$0 \leqslant \varliminf \leqslant \varlimsup \leqslant 0$$

we immediately deduce that

$$\varliminf = \varlimsup = \lim = 0,$$

which gives the desired result.

If x is complex (with a positive real part), slight modifications have to be made in both of these arguments ; but their relative complexity is unaffected.

An essential feature of both (A) and (B) is that they both involve inequalities of the form

$$0 \leqslant e^{-t} - \left(1-\frac{t}{n}\right)^n \leqslant K\phi(n)f(t) \tag{4}$$

with $\int_0^X t^{x-1}f(t)\,dt$ convergent for any fixed positive X, and

$$\lim_{n\to\infty}\phi(n) = 0\ ;$$

$\phi(n)$ and $f(t)$ being independent of t and n respectively, while K is independent of both t and n.

The reasoning used in (A) is applicable when

$$\int^\infty t^{x-1}f(t)\,dt \tag{5}$$

is convergent ; but, when this integral is not convergent, I cannot see how reasoning substantially equivalent to that used in (B) is to be avoided. The device of taking X to be a function of n which tends to infinity sufficiently slowly when $n\to\infty$ may be used, but, even so, the range of integration still has to be divided into three parts, each requiring separate treatment.

One would suppose that the authors of *Modern Analysis* described (1) as less precise than (2) because (5) is convergent when $f(t)=t^2e^{-t}$ and it is not convergent when $f(t)=t^2$. Thus the inequality (2) constricts the function $e^{-t}-(1-t/n)^n$ just where constraint is required to make the reasoning of (A) applicable, like the pre-war feminine garments of metal and whalebone which were of the precise shape required for producing wasp-like waists. The constant K in (4), like the lace frills on these garments, plays no part in applying any effective constraint, and, so far as both (A) and (B) are concerned, its value is irrelevant.

An enquiry into the source of (2) revealed that it was appropriated (without acknowledgment) from a paper in the *Messenger*, 45 (1916), 28-30; but the word used there was "powerful" not "precise". Perhaps the associations of the word "parful" in Ch. XXII of E. F. Benson's *The Babe, B.A.*, made the authors of *Modern Analysis* decide against using it in a book likely to fall into the hands of undergraduates.

In conclusion, as a peace-offering, I tender to E. H. N. the inequality

$$0 \leqslant e^{-t}-\left(1-\frac{t}{n}\right)^n \leqslant \frac{t^2(1+t)e^{-t}}{2n}; \quad (0 \leqslant t \leqslant n,\ n \geqslant 2)$$

he should have no difficulty in proving this result, and also in convincing himself that the expression on the right does not exceed the expression on the right of (1) in the range $0 \leqslant t \leqslant n$ when $n \geqslant 2$, and that this inequality can be used in conjunction with arguments of type (A). G. N. W.

1255. *American tournaments.*

With reference to Note 1213 (XX, p. 333), an alternative way of giving the whole of the draw for an American tournament is illustrated by the following diagram in which letters denote teams and numbers refer to rounds in which two teams meet.

	A	*B*	*C*	*D*	*E*	*F*
A	–	1	2	3	4	5
B	1	–	3	4	5	2
C	2	3	–	5	1	4
D	3	4	5	–	2	1
E	4	5	1	2	–	3
F	5	2	4	1	3	–

e.g. C plays D in round 5.

The numbers in every row and column are in numerical order except that the numbers which should appear on the blank diagonal are removed to the bottom row and right-hand column.

For an odd number of teams omit the last column and row.

M. A. Porter.

REVIEWS.

Topologie I. By P. ALEXANDROFF and H. HOPF. Pp. xiv, 636. RM. 46.80. 1935. Die Grundlehren der Mathematischen Wissenschaften, 45. (Springer)

About the year 1925, topology was in the uncomfortable condition of falling into two scarcely connected parts, which had for some time threatened to become completely separate subjects. These were the theory of sets of points in general topological spaces—" point-set topology "—and the algebraic theory of the connectivity properties of manifolds and complexes—" combinatorial topology ". These two subjects had distinct origins, the first in the work of Cantor, Borel, and others, the second in Poincaré's famous series of memoirs, starting in 1895, on the Analysis Situs of n-dimensional manifolds. In the beginning the only connection between the two branches was that the object of both was to discover " topological " properties—properties that are invariant under a (1,1), both ways continuous, mapping. Poincaré's rather intuitive treatment was soon felt to be logically unsatisfactory, and it was clear that to provide the combinatorial theory with a rigorous basis some appeal must be made to point-set theorems and methods. This difficult problem was solved, in the astonishingly simple way that is now familiar, by the work of Brouwer and Alexander. A further step was the Duality theorem of Alexander, which showed that Jordan's theorem, which originally seemed to be on the point-set side of the gap, really belonged to the combinatorial side. So far, however, the approach had been entirely from one side: point-set theorems of a comparatively simple kind were used in the Theory of Complexes, but the point-set theory itself pursued a course quite untouched by combinatorial methods.

This situation was completely changed by papers that began to appear about 1925, by Vietoris, Lefschetz, but above all by Alexandroff, in which, on the one hand, the problem of extending the concepts and results of the Theory of Complexes to general topological (or at least general metrical) spaces was systematically undertaken; on the other hand, theories that had been regarded hitherto as definitely in point-set territory (in particular the Theory of Dimension) were fruitfully attacked by combinatorial methods. An important part in this development was played by Alexandroff's method of approximating to metrical spaces by an abstract complex called the *nerve*, determined essentially by the way in which a system of closed sets, covering the space and of small diameter, fitted together. Lebesgue's famous " Pflastersatz " was the basis of this method.

By the time this unification began both sides of Topology had attained a high pitch of development. It is therefore not surprising that the combinatorial theory of general spaces, which takes both sides of the subject, at more or less their 1925 levels, as starting point, should be beyond the scope of any ordinary book for students, and in fact (though Lefschetz's *Topology* (1930) has a chapter on open manifolds) no textbook has hitherto attempted any systematic account of the blended theory. (The excellent Encyclopedia article of Tietze and Vietoris has of course no pretensions to be a textbook. It is an account of the state of knowledge in 1931, with references but not proofs.) Nevertheless to stop short at this stage is to give a most inadequate idea of modern topology as a unity. Alexandroff and Hopf have therefore set about repairing this omission by writing a treatise on such a scale that they will be able to give an account of the main lines of the entire theory, an account, as they say in their Preface, " not of the whole of topology, but of topology as a whole." Their framework is a work in three volumes, of which the first, now published,

has 616 pages of text, This volume is devoted mainly to the Topology of Complexes: a summary of its contents is given at the end of this review.

Besides the mere extent of their material, a further difficulty of presentation the authors had to face is the steadily increasing *abstractness* that has characterised recent work in combinatorial topology. The first symptom of this movement was the gradual retirement of the Betti *numbers* and torsions, as fundamental invariants, in favour of the Betti *group*, of which they are parameters. This was a natural result of the consideration of spaces with infinite connectivities, but it also fitted in well with the desire to exhibit as clearly as possible the purely algebraical content of each topological theorem. The Betti groups include not only the ordinary Abelian groups arising from k-chains with integer coefficients, but also the groups "mod m" considered by Alexander, and the groups "with division" which arise in connection with looping coefficients. When the algebra began to be scrutinised on its own account it was natural to fit these groups as special cases into a theory of chains with coefficients in any ring, or even, as was found to be practicable in the greater part of the theory, in any abstract Abelian group, $\mathfrak{J}$. A final generalisation is the simultaneous consideration of $\mathfrak{J}$-cycles and $\mathfrak{J}'$-homologies, where $\mathfrak{J}'$ contains $\mathfrak{J}$; and one is thus led to the "$(\mathfrak{J}, \mathfrak{J}')$ Betti group" of a complex. (Thus if $\mathcal{J}$ and $\mathcal{R}$ are the rings of integers and rational numbers respectively, the $(\mathcal{J}, \mathcal{R})$ Betti group is that which arises from "homology with division" in the usual sense.) These theories are by no means a case of generalisation for its own sake: coefficients in $\mathcal{R}_1$, the group of rational numbers mod 1, which were hardly considered before, have played a most important part in Pontrjagin's analysis of the group theory behind the Alexander Duality theorem.

For the authors to succeed in their design of giving a view of topology as a whole it was obviously necessary that their book should not be a work of reference, but one to be read through as a whole. Their hardest problem in dealing with such a mass of material was therefore one of *tempo*, to produce an account which should really contain every step in the argument, but should not stagnate in masses of detail on the way. In solving this difficult problem they have been brilliantly successful; in spite of the large scale of the work their treatment is not only lucid in detail, but at every point it is clear what part the present argument plays in the general development. To this purpose of "keeping the theory moving" everything has been made subservient. The way to avoid long and disheartening proofs is to cut them up into parts, each of which can be fitted into a separate part of the theory. This process has been used throughout the work. In particular the group theory has been entirely separated from the rest and set out in an appendix. Diagrams are plentiful, and examples are given whenever they are needed to elucidate the text, but there are no larger scale examples, "for their own sake"; they would hold up the main argument. The result is a book which, though it assumes no actual theorem of topology, is not for beginners. The segregation of the algebra underlying such a theorem as the Duality theorem tends to make it harder to grasp its geometrical meaning. The form in which the Betti-group theory is presented is uncompromisingly abstract, and to keep a hold on the properties of the groups, with respect to all the various combinations of coefficients that are considered, one needs to have already formed a fairly settled idea of the simplest geometrical connotations of connectivities in 2 and 3 dimensions. But the authors were clearly right to throw over the absolute beginner. Without the sacrifice of "easiness" the book would almost certainly have lost its extraordinary fluency, and through trying to appeal to two kinds of reader would have been

satisfactory to neither. As it is, and especially if the authors are equally successful with the still more difficult programme for Volumes II and III, research workers in topology who " know a little already " will have in their possession a superb guide to the whole domain.

The only criticism I wish to make is one of detail. It is perhaps carrying the determination to deal with groups and not with numbers a little too far to exclude the torsion coefficients (as distinct from the torsion group) from all but a passing mention. Not only have these numbers important applications, *e.g.* in the classification of knots, but the authors themselves have often to refer to numbers f_i^r which are allied to the r-dimensional torsions, and in nearly all the contexts replaceable by them. (Cf. in particular pp. 226-33, and p. 438.) These numbers have either to be defined afresh each time, or to be referred to by some such phrase as " the numbers defined above ", which really gives the reader more trouble than would the little extra group theory required for the invariant-factor theory.

The following is a very brief outline of the contents of Volume I. First comes a concise but very clear account of the general properties of topological and metrical spaces, with special emphasis on the property of compactness. The second part contains the theory of the Betti groups of complexes—not only complexes in the usual elementary sense, called by the authors " polyhedra ", but also the general complexes in an " Eckpunktbereich " introduced by Alexandroff; and of course with the general coefficients already described. In the third part the invariance of the Betti groups is proved first by Alexander's method of simplicial approximation (for " polyhedra " only), and secondly by considering the " nerve " of the space (for any compact metrical space). This second method also leads to a simple proof of the Lebesgue Pflastersatz, and of the simplest theorems of the Theory of Dimension. There is also in this part a proof of the " Theorem of Division ": if F is a closed set in the euclidean space R^n, $p_0(R^n-F)=p_{n-1}(F)+1$, (where p_i is the ith Betti number). The dominating idea of Part 4 is the looping-coefficient of two cycles, and the main result the Alexander Duality theorem, with Pontrjagin's proof. (At several points use is made of Borsuk's " retracts " in passing from " straight " to " curved " cases.) There are also chapters on the Brouwer degree of a mapping, on homotopy properties of mappings, and on fixed-point theorems. Although there are novelties of proof and treatment in every part of the book it is this fourth part that contains the most substantially new results. Chapter XIII especially contains important theorems on " essential " and " inessential " mappings, and on the homotopy-classes of mappings of complexes in an n-sphere, on the lines originated by Hopf.

There is finally a first appendix, already mentioned, in which the relevant part of the theory of Abelian groups is set out with the most admirable clearness; and it is agreeable to find in a second appendix those properties of convex cells in euclidean space that are usually either assumed without proof, or else allowed to hold up the argument.

The authors avoid committing themselves deeply on the intended contents of the remaining volumes, but it appears that Volume II will contain the theory of Betti groups, Duality, etc., in general spaces, and general Dimension Theory; and Volume III the theory of manifolds, including the fundamental group.

What appears to be a slip in Chapter IV is perhaps worth mentioning, since the intended meaning is obscure for a page or two. The definition of an HS^n on p. 202 does not give the intended meaning when $n=0$; for it is clear (see *e.g.* middle of p. 215) that an HS^0 is meant to be a pair of points, which is not monocyclic.

M. H. A. N.

Moderne Algebra I. 2nd edition. By B. L. Van der Waerden. Pp. x, 272. Geh. RM. 15.60; geb. RM. 17.20. 1937. Grundlehren der Mathematischen Wissenschaften, 33. (Springer, Berlin)

Moderne Algebra by Van der Waerden established itself as a classic soon after publication in 1931, its popularity necessitating the recent issue of a second edition. The high reputation of the book is due in part to the unified general way in which the enormous mass of detailed and specialised knowledge, in widely differing branches, is made accessible to mathematicians. The ideas are clearly and simply expressed and although it cannot be regarded as a textbook in the ordinary sense for the word, it would form an admirable basis for an undergraduate course in mathematics if it were supplemented by examples from topology, algebraic geometry, differential equations and Boolean logic.

There is little change, in this second edition, from the original. Chapter I is an adequate introduction to number, and Chapter II, in which is incorporated an additional article on group complexes, to abstract groups. Chapter III, which deals with abstract rings and fields, contains a new article introducing vectors and hypercomplex numbers. Chapter IV is concerned with the theory of polynomials, symmetric functions in a ring or field, the decomposition into prime polynomials being treated in detail. Two articles on the resultant, which formerly appeared in the second volume, are now included here. Chapter V, which discusses the simple yet important ideas of Steinitz on the structure of fields, is one of the most interesting in the book, illustrating as it does the difference in the standpoint of modern algebra and the algebra of Weber and Chrystal. The article on the solution of linear equations in non-commutative fields was originally in the second volume of the first edition. Chapter VI, which is unaltered, develops further group theory including the generalisations of the automorphisms of a group termed operator isomorphism. Chapter VII discusses at length the Galois theory of equations and includes an article on constructions with ruler and compass. Chapter VIII is on algebraically closed fields and simple transcendental extensions. It contains a new article on the differentiation of algebraic functions. Chapter IX, which is devoted to real fields, includes the discussion of real numbers. Chapter X, the final chapter, gives an account of the p-adic extensions of a field based on the theory of Archimedean and non-Archimedean absolute values.

A. R. R.

Theory of Linear Connections. By D. J. Struik. Pp. vii, 68. RM. 8.60. 1934. Ergebnisse der Mathematik, Band III, Heft 2. (Springer, Berlin)

This is a brief but fairly thorough exposition of the modern tensor algebra and its geometrical applications which have arisen in connection with the Relativity theory and more lately with the theory of spinors. The whole treatment is very abstract; algebraic ideas are fundamental, geometry is explicit but of the most formal, physical applications are not hinted at. The work is rather on the scale and in the style of a Cambridge Tract, and gives a comprehensive survey of the topic; but it must be said that it seems too condensed to be tolerably easy reading to anyone who does not know a good deal of the matter already. The literary style, and the mathematical style, have the concise formality of German mathematicians, and the notation is very complicated, perhaps unavoidably so in view of the range of different geometries covered; suffixes appear in four positions instead of the familiar two, and dashes precede as well as follow the symbols which they affect. The page or less at the end headed "Notations used" is not very helpful except as a reminder to one who has already read most of the book.

On the other hand few are likely to want to master this subject who have not

already some knowledge of at any rate Riemannian geometry, and the affine connections, and with this much preparation Dr. Struik should be more or less intelligible. He leads the reader on from here to the Hermitian and projective connections, by means of Veblen's " geometry of paths ", and deals also with the geometry induced in a manifold by being embedded in another whose geometry is known.

There are nearly eleven pages of bibliography, chronologically arranged and, as far as I can see, very far reaching and carefully constructed.

PATRICK DU VAL.

Sub-harmonic Functions. By T. RADÓ. Pp. v, 56. RM. 6.60. 1937. Ergebnisse der Mathematik, Band V, Heft 1. (Springer, Berlin)

The idea of the sub-harmonic function had its origin in the abstraction of the essential property of such functions from special types of functions studied particularly. Although the use of the term " sub-harmonic " has had a unifying influence upon thought and method, no unified account of the theory has appeared prior to the appearance of this slim volume, and a knowledge of the theory has generally been obtained by stumbling across results of sub-harmonic type in particular fields of analysis. This volume sets out all the available information concerning sub-harmonic functions in condensed form. One hardly realized how much detail had been added to the main structure of the theory by American mathematicians, and the author is to be congratulated upon the way in which he has never allowed his work to become a mere catalogue in view of the condensation demanded by restrictions upon space.

The first two chapters are devoted to the standard alternative definitions and the standard results concerning integral means upon which interest in the subject originally concentrated. The author has set down detailed results under extended conditions which will be new to many students of analysis in this country, for we English have been in the main content with the broad outline of the theory. The third chapter entitled " Criteria and Constructions for Sub-harmonic Functions " is all devoted to essential properties. Chapters V and VI are devoted to the connection between sub-harmonic functions and general potential function theory. A final chapter gathers together a number of miscellaneous results and the volume closes with an admirable bibliography.

The author has rendered mathematics a great service in placing together in one volume important information which has previously remained scattered over many papers by many authors. His style is clear and easy to follow, and the volume does not suffer from over-compression.

R. M. G.

Lehrbuch der Ballistik. Ergänzungsband. By C. CRANZ. Pp. xii, 292. RM. 36. 1936. (Springer, Berlin)

This volume is supplementary to three volumes previously published: External Ballistics, 1925; Internal Ballistics, 1926; and Experimental Ballistics, 1927; and is divided into three parts corresponding to these subjects. Its appearance is due to the fact that, since the previous volumes were published, ballistic science has developed appreciably and interest in the subject has recently revived, at all events, in Germany.

In Part I the author deals first with recent developments in the theory of the resistance of the air to projectiles, particularly the recent application of gas-dynamics to the formation of the conical wave-front at the point of a conically-headed projectile. Dynamic similarity is applied at some length and the so-called form-coefficient is critically discussed. Recent methods of solving the equations of motions of a projectile are next considered, including Popoff's

application of Poincaré's method of integration, whereby a solution is obtained in rapidly-converging series. Step-by-step methods are also reviewed, particularly special methods for vertical and long-range trajectories which reach the stratosphere.

The effect of air temperature on air resistance is next discussed. The effect is two-fold; there is the proportional variation due to the variation in density, and there is also the more complicated variation in elasticity. The latter has its maximum effect at velocities approximating to the velocity of sound in air. It is generally considered that the viscosity effect on resistance due to temperature is negligible at the high velocities under consideration.

Langevin's transformation of coordinates is used by O. v. Eberhard to develop a new generalisation of the Euler-Otto tables, and the errors of Stübler's differential formulae (given in Volume I) are discussed. The general effect of a cross wind is considered and the influence of the rotation of the earth on the drift is outlined. The first part concludes with a comparison of results of calculting trajectories by various methods.

In Part II the theory of air waves of finite amplitude is applied to determine the resistance of the air to the shot during its motion along the bore; it is also applied to determine the limiting velocity of propulsion of a given propellant. The difference between burning and detonation is discussed; the rate of burning is a variable depending on local physical conditions; the velocity of detonation is constant, being the velocity of a compression wave through the material of the explosive before disruption.

The second part concludes with a proposal for the complete theoretical solution of the principal internal ballistic problem, which is, to determine the velocity of the shot and the mean pressure of the gases at any position of the shot in the bore. There is a notable omission in this part; no reference is made to recent developments in gun construction in the direction of auto-frettage.

Part III is not of so much interest to mathematicians. It deals mainly with the applications of the resources of modern physical apparatus to experimental work in ballistics. Among these may be instanced the use of the piezo-electric pressure gauge for the measurement of high pressures of short duration; the cathode-ray oscillograph, photo-electric cells, thermionic valves for measuring and recording short intervals of time.

The book concludes with a miscellaneous collection of ballistic problems with their solutions.

F. R. W. H.

Vorlesgungen über Integralgeometrie I-II. By W. Blaschke. Pp. ii, 1-60; ii, 61-128. Wrappers, RM. 3.75, RM. 3. 1936, 1937. Hamburger Mathematische Einzelschriften, 20, 22. (Teubner)

Integral Geometry may appear at first sight an unfamiliar term to many mathematicians, but most of the matters with which it deals are familiar under other names. Thus many of the questions discussed in these two volumes arise in problems belonging to the field of geometrical probability. The well-known " Needle-Problem " of de Buffon, for instance, involves geometrical investigations of great interest which, however, seldom appear in ordinary textbooks. Such problems as these are treated by Professor Blaschke. What he has done, in fact, is to exhibit the basic geometrical principles which underlie a number of special investigations in this and other fields as a logical sequence which has a genuine intrinsic value.

The two volumes under review deal with properties in euclidean two- and three-dimensional space of loci in euclidean space. The general method is to introduce generalised notions of density, for instance the density of lines and

planes in euclidean space, and to apply these notions consistently to a large collection of problems. Thus the properties of planar regions bounded by oval curves are deduced from the consideration of the integral of the density of a line over a suitable set of lines in the plane. To give a list of the results and applications which can be derived by such means would be to recount the tables of contents of the two volumes. It is a sufficient illustration to mention that by these means Santaló derived the well-known formula

$$L^2 - 4\pi F \geqslant 0$$

connecting the length L of a closed plane curve and the area F enclosed by it, in an extremely agreeable manner. In his preface to Volume II Professor Blaschke expresses regret that he has not yet succeeded in obtaining the corresponding relation

$$F^3 - 36\pi V^2 \geqslant 0$$

connecting the area F of a closed surface and the volume V of the enclosed region. Nevertheless he is confident that his methods are sufficiently powerful to prove this result.

We can confidently recommend this work to a large circle of readers. It does not require any elaborate preliminary reading before it can be opened. It is, rather, a book which should be intelligible to any competent mathematician, and it will give considerable pleasure to all but the mathematical snobs. Of the style we need only say that it is as lucid as any of Professor Blaschke's writings. W. V. D. H.

Projektive Liniengeometrie. By R. Sauer. Pp. 194. RM. 9. 1937. Goschens Lehrbücherei, 23. (Walter de Gruyter, Berlin)

A glance through the indexes of the last ten volumes of the *Proceedings* of the London Mathematical Society reveals the interesting fact that not a single paper on projective differential geometry appears in these volumes, and this in spite of the fact that geometry is generally regarded as a flourishing branch of English mathematics. Perhaps the inference to be drawn from this rather curious phenomenon is that our geometers have tended to divorce the synthetic and differential aspects of their science, and they have probably been assisted in this by the fact that most of the current English works on differential geometry seem to regard this as a self-contained branch of mathematics, and tend to stress the analytical rather than the geometrical aspects of the subject.

The interesting volume under review is written from an entirely different point of view. Its title may mislead English geometers into expecting a treatise on algebraic line-geometry, whereas in fact it is the differential aspects of the subject which form the main topic of the work. But although the methods used are necessarily to a large extent analytical, it remains a work of geometry first and foremost, and as such deserves the attention of every geometer. It is fairly elementary in treament, and presupposes nothing that should not be part of the stock-in-trade of any mathematician; and its first chapter summarises concisely certain familiar ideas about line-coordinates. The dominating idea throughout the book is that of representing the six coordinates of a line as the components of a vector p, and defining the scalar product pq of two such vectors as the mutual invariant of the corresponding lines. Thus the vanishing of pq is necessary and sufficient in order that the corresponding lines should meet, and, for any line, $pp=0$. Essentially, of course, this is the familiar representation of lines in five-dimensional space, but the author makes no use of this correspondence in the book. This is perhaps unfortunate, as many of his results are intimately bound up with differential geometry of curves on a quadric in five

dimensions, but in any case, the reader who is familiar with the representation will be able to consider these relations himself.

After these preliminaries, the author devotes his second chapter to an interesting and illuminating account of the projective differential geometry of ruled surfaces. The lines forming the surface are represented by vectors p, where p is a function of a scalar parameter u. The fundamental distinction is made between developables and scrolls. If a surface is developable, then $\dot{p}\dot{p}=0$ where $\dot{p}$ is the derivative of p with respect to u. (The initiated will at once see that this implies that the corresponding curve in five dimensions has its surface of tangents lying entirely on the basic quadric.) For skew surfaces we can define various osculating line-systems (need we add that they correspond to the osculating spaces of the corresponding curve in five dimensions?), beginning with the osculating regulus. This is defined by three consecutive generators, and the lines of the complementary regulus are inflexional tangents of the scroll. The osculating linear congruence contains four consecutive generators, and its two directrices are flecnodal tangents of the surface, having four point contact. These tangents form a second scroll, and the two scrolls are symmetrically related. After considering certain degenerate cases (for example those for which the two flecnodal tangents associated with a generator coincide) the author goes on to consider the invariants of ruled surfaces. For this purpose he shows that we can choose a certain function s of u which remains invariant under change of parameters; s is thus analogous to the arc-length of a curve, but its definition is purely projective. This forms a "natural" parameter for the system of lines. There are three independent functions of s which remain invariant under arbitrary projective transformations of the surface (supposing it to be of a sufficiently general type). Applications are given to scrolls with infinite groups of projectivities into themselves, including as special cases the cubic scroll.

It is impossible to describe the rest of the work here in the same detail. Suffice it to say that the same basic ideas are applied to congruences and to complexes (which, depending on more parameters, are somewhat less elegant to handle), and with applications to the theory of the deformation of surfaces. Those readers whose appetites have been whetted by the chapter on ruled surfaces will soon discover for themselves that the subsequent chapters are no less full of interest.

Much of the subject matter of this book is quite old (though still more of it was new to the present reviewer), and the book is expressly written in an elementary way and addressed to the young mathematician. It is not intended, therefore, to be an exhaustive treatise, but it does set out the main ideas in a very clear manner. Those geometers to whom projective differential geometry has hitherto seemed something recondite and mysterious will find much to interest them in this volume, and they will not regret the time they have spent in reading it. J. A. T.

Scientific Inference. By Harold Jeffreys. Re-issue with addenda. Pp. vii, 272. 10s. 6d. 1937. (Cambridge)

The first edition appeared in 1931, and was reviewed by W. L. Ferrar in the *Gazette* XVI, p. 58 (February 1932). The present edition differs from the first only in the addenda pp. 244-69, but the additions concern matters of fundamental importance. To explain these it will be necessary to make a brief reference to the older portions. These consisted of eleven chapters and three appendices. Chapter VII (Mensuration) and two of the appendices dealt with questions in pure mathematics, and Chapters VIII and IX with mathematical physics (Newtonian Dynamics, Light and Relativity), while Chapter VI

(Physical magnitudes) was concerned with both. The rest of the book expounded the theory of probability due to the author and Dr. Dorothy Wrinch, of which two of the postulates are, "it is possible to learn from experience and to make inferences from it beyond the data known by sensation", and "the order of decreasing simplicity among laws is also the order of decreasing prior probability" (the simplicity postulate).

The addenda open with a quotation from Keynes: "After all, there is no harm in being sometimes wrong—especially if one is promptly found out." It is admitted that for some purposes the simplicity postulate is too vague, and that attempts to make it more precise have led to unacceptable consequences. The treatment of errors is admitted to rest upon an incomplete analysis. Apparently Dr. Jeffreys' theory of probability has been largely reconstructed. A brief summary is given on pp. 250-52, but it will probably be necessary for the reader to consult Dr. Jeffreys' recent original papers (a list of 13 recent researches is given on p. 269) to obtain a clear idea of the theory. This is very disappointing, as it appears to have important applications.

Many of us have never been quite clear about the logical foundations of the work of Prof. R. A. Fisher, although we believe, largely on empirical grounds, that his methods are substantially correct and of great practical importance. Dr. Jeffreys tells us that "Fisher's practice does not follow from his postulates, but it, or something very like it, follows from mine", and then he tantalizes us by omitting the evidence for this statement. There is no mention of the method of testing statistical hypotheses due to J. Neyman and E. S. Pearson.

It will be lamentable if the science of statistics is going to split up into a collection of sects. May we appeal to Dr. Jeffreys to give us, in convenient form (possibly as an article in the *Gazette*), a unified Jeffreys-Fisher-Neyman & Pearson theory? Will he, in the next edition, include among his amusing chapter headings:

"Birds in their little nests agree,
And if they can, then why can't we?"

H. T. H. P.

Thermodynamic Theory of Affinity. A book of principles. By Th. De Donder and P. Van Rysselberghe. Pp. xx, 142. 13s. 6d. 1936. (Stanford University Press; Humphrey Milford)

If a reaction is taking place in a closed thermodynamic system its "degree of advancement" can be measured by a parameter ξ, introduced by De Donder. With an infinitesimal process in which ξ changes to $\xi + d\xi$ there can be associated an "uncompensated" heat Q', in the sense of Clausius. The hypothesis introduced by De Donder is that the "affinity" A, $\equiv dQ'/d\xi$, is a function of the state of the system only, and does not depend on the particular infinitesimal process. The present monograph is devoted to the working out of the consequences of this hypothesis, together with a number of general thermodynamic formulae connected with them.

The reviewer confesses that he does not see the point of this hypothesis. For pure thermodynamics in its current form is a complete theory in the sense that it gives (apart from possible manipulative difficulties in the mathematics) a complete solution to every problem which can be stated in purely thermodynamic terms. Classical dynamics is another example of a theory which is complete in the same sense. So one cannot see how pure thermodynamics can find room for any new hypothesis of this sort, any more than classical dynamics can. The question whether the hypotheses of the subject should be superseded by others, as classical dynamics may be superseded by general relativity theory, is beside the present point. The answer that the new hypothesis is made to give information about "velocities of reaction" is illusory. For

neither it, nor current thermodynamic theory, can give information about *rates* of change, but only about the *direction* in which change will proceed, since all thermodynamic concepts are defined only for equilibrium, or so-called " quasi-equilibrium ", states.

If on the other hand the reviewer is missing an essential point, he cannot be altogether blamed for doing so. For no explanation is given of the principles which have suggested the introduction of the central hypothesis. Whatever justification there may be for adopting this method in regard to the basic hypotheses in an axiomatic treatment of a subject, it surely does not apply to additional hypotheses brought in at an advanced stage in its development. The work in fact belies its sub-title, " A Book of Principles." It is much more a book of formulae. The purely mathematical derivation of these formulae is indeed set out with admirable clarity. W. H. McCrea.

College Algebra. By C. I. Palmer and W. L. Miser. 2nd edition. Pp. xvi, 467. 15s. 1937. (McGraw-Hill)

Written for use in the freshman year of American colleges and technical schools this work has every appearance of being a suitable and workmanlike production.

In less than 400 pages of text it provides a course beginning with the rudiments and including such topics as De Moivre's theorem, Determinants, Theory of Equations with Horner's method, Convergence. There are 14 pages of tables (without difference columns), including trigonometrical functions which retain the characteristic 9 instead of $\bar{1}$, and annuity tables. There is a very complete index of 7 pages and 45 pages of answers.

The earlier chapters revise the elementary parts of the subject, emphasising the application of the fundamental laws, though elsewhere the bias of the book seems to be practical; for example, considerable attention is paid to annuities, the actuarial symbolism being employed. Frequent historical notes are inserted.

In certain places the authors depart from their apparent intention to be severely logical; for example, on p. 15 occurs, " It follows from the meaning of a negative number that the sum of two numbers of the same absolute value is zero." Hence they prove that $a(-b)=-ab$. But the meaning of negative numbers has not been given. On p. 192, to prove that $\sqrt{15}+\sqrt{24}>\sqrt{35}+\sqrt{8}$ the authors start by assuming the statement is true, reduce it to $19>\sqrt{280}$ and instruct the reader to reverse the steps. But they give no caution about the validity of reversing steps. This is perhaps more striking considering the grandfatherly attitude of the authors which finds expression in exhortations such as: p. 73 (solving equations), " Here blind adherence to set rules will not suffice but a little attention to a few simple principles will remove all difficulty "; and p. 334 (Horner's method), " While general directions for performing the work can be given, great care should be taken in all numerical computation and in drawing conclusions as there are many pitfalls for the careless."

Meanwhile the pupil in need of such exhortation is expected to understand, p. 214 (proportion), "The subject is of the greatest importance in measurements in the physical sciences where the magnitudes are not measured directly but indirectly. That is, a direct comparison is not made between the magnitude to be measured and a unit of the same kind; but between two magnitudes of different kinds, which are proportional to the magnitude to be measured and its unit."

The book is well got up and in spite of the amount of matter introduced there is no appearance of compression, and there are ample exercises; but it would be difficult to assign for it a suitable place in English education.

F. C. B.

Puzzles Papers in Arithmetic. By F. C. Boon. Revised and enlarged edition. Pp. 64. 1s. 6d. 1937. (Bell)

The first edition of these excellent puzzles was reviewed in the *Gazette*, Vol. XIII, p. 178, and contained 37 papers. These have now been reprinted and extended so that the present edition contains 48 papers each of six questions. The book opens with some hints to solvers stressing the importance of systematic trial and the value of thoughtful tabulation. There is an appendix on magic squares and another on some interesting properties of numbers. Answers are given to the majority of the puzzles.

The author's claim that the " pupils evince a sprightlier attitude which quickens their pace in the formal work " is amply justified and the intriguing nature of the problems contributes largely towards this. It is no exaggeration to say that every teacher of school mathematics should use the book regularly.

W. J. L.

Arithmetic. By C. H. Hill and P. G. Welford. Books I, II and III. Pp. 1-143, 145-263, 265-383. With Answers 1s. 9d.; without Answers 1s. 6d. each. 1937. (University Tutorial Press)

" This entirely new work is intended for use in Senior, Central and Secondary Schools and constitutes a four or five year course up to the end of the School Certificate year." These books form the first three parts of this course and are intended to cover all the ground ; the fourth part, in preparation, is a revision book for School Certificate. The books are clearly printed, a fact which is stressed in the preface, and are well bound.

There are plenty of revision tests and Part III contains a set of 138 questions taken from School Certificate examination papers. The routine examples are often disappointing. No one feels any urge to begin a set of six or more problems which are identical in form and deal merely with different sets of magnitudes. Worse still in many cases here, the words common to the questions are denoted by commas in all but the first. Logarithms do not appear though Graphs are carefully treated. Standard Form is used in the treatment of Decimals ; multiplication is by Method IVa and Division by Method IIb of the Arithmetic Report. Contracted methods in decimals are dealt with very fully ; the explanation in each case takes about two pages.

The treatment all through is adequate without being inspiring. The authors show, by the carefully arranged worked examples, that they wish to help the pupil to a fuller understanding of the processes and to secure something more than a mass of figures as the answer to every problem in arithmetic. It seems that they would have achieved more by leaving the teacher greater freedom, especially in the early work, and by omitting many of the rules.

W. J. L.

A School Algebra. III. Pp. viii, 289-494, 1-23. 3s. 6d.

A School Algebra. Certificate Course. Being parts of *A School Algebra, II* and *III*. 4s. 6d. By R. M. Carey. 1937. (Longmans, Green)

It is difficult to know exactly what is implied by a " Certificate Course " in any subject, but if it means in general the " public school " as opposed to the " preparatory school " course, it would seem advisable that the course should commence with some revision of earlier work, especially of Symbolical Expression, Equations and Directed Numbers. This particular volume contains chapters on Multiplication and Easy Factors, Simultaneous Equations, Problems, Trinomial Factors, Quadratic Equations, Further Factors, Revision of Fractions and Equations, and Miscellaneous Problems. The first section of the book closes with a rather lengthy collection of revision papers (covering 75 pages of print).

All the above topics are treated in a more or less conventional manner, but throughout the work a certain abruptness is apparent, especially in illustrative examples designed to introduce new ideas. Thus, in discussing the multiplication of $(3x+5)(2x+7)$ we find: "... the middle term is easy and the following diagram helps:

$$\overset{\frown}{(3x+5)(2x+7)}."$$

This would form an excellent revision device, but no mention is made anywhere of the fundamental intermediate step $3x(2x+7)+5(2x+7)$, which affords the logical explanation for the middle term. Or again (p. 160) on the initial stages of the work on quadratic equations:

$$\begin{aligned} \text{" Solve } (x-3)^2 &= 5. \\ x-3 &= +\sqrt{5} \\ \text{or } x-3 &= -\sqrt{5}\ldots\text{"}, \end{aligned}$$

with no other explanation or reference to the connection between this method and the difference of two squares. This defect, while not perhaps serious if the book is handled by an experienced teacher, is likely to render it less effective in the hands of the young or unskilled.

There are some topics which make only an accidental appearance, chiefly in the long sets of revision examples referred to above. Prominent among these topics are Graphs, Functionality and Symbolical Expression. This, again, renders the teacher's task more difficult, since, in order to ensure the completeness of his course he must make the correct selection of revision papers.

Part III opens with a chapter on Variation with which is associated a section on the general shapes of various graphs, and continues with sections on Surds, Indices, Logarithms, Ratio, Series, Harder Equations, Formal Algebra, Permutations and Combinations, the Binomial Theorem. It concludes with a chapter on the Calculus. In the *Certificate Course* the last four of these chapters are omitted. The treatment of these topics is, in the main, conventional and the style follows the lines of the previous part. Two quotations will illustrate:

(i) After a statement of the laws regulating positive indices, the bookwork proceeds: "These laws are, however, true for all values of m and n, positive, negative, rational and irrational; the proofs are, however, beyond the scope of this book."

(ii) " Express

$$\frac{x^2+5}{(x^2+1)(x-1)^2}$$

in partial fractions. The partial fractions will be

$$\frac{Ax+B}{x^2+1}+\frac{C}{(x-1)^2}+\frac{D}{x-1}."$$

This is not preceded by any discussion on the formation of such expressions and the special difficulties connected with repeated factors and irreducible factors.

The sections on Permutations and Combinations and the Binomial Theorem are rather sketchy; for example, the method of finding the greatest term in a binomial expansion is not mentioned, and though the Binomial Series for n negative or fractional is given, no reference is made to convergency, except a rather vague statement about the "difficulty" of evaluating such a series if $x > 1$.

In the chapter on Calculus the usual topics (up to volumes of revolution) are treated, for powers of x only. Most of this section seems to be adequate, but it is a pity that it should be tacitly assumed, without precise statement, that the derivative of the sum of several functions is the sum of their derivative and that the derivative of a constant is zero.

Amongst smaller points the following may be noted. A wrong formula is given for the sum of a geometric progression and the symbolism " $n \to \infty$ " is introduced without any explanation. In the section on series the result for $\sum_1^n r^2$ is given, with the note that a proof appears in Ex. 21 e. This seems to be a false clue, as is also the hint given for the summation of $1^2+3^2+5^2+\ldots$ in this exercise. A similar flaw occurs later where the discussion of maxima and minima introduces the d^2y/dx^2 rule, a proof being promised in Ex. 26 d (which should read Ex. 26 e). The section on integration introduces the symbol $\int$ but no reference is made to the connection of integration with summation.

The printing and arrangement throughout are excellent and there is no shortage of examples. G. L. P.

Examples on Practical Mathematics. Third Year (Senior Course) for technical colleges. By L. TURNER. Pp. 112. 2s. 1937. (Arnold)

This is the third of Mr. Turner's excellent collections of examples for students taking practical mathematics in technical colleges ; this collection is intended for the year in which they sit for the ordinary national certificate examination. The type and variety of the examples could hardly be improved. I do feel, however, that the proportion of fifty pages on the calculus to twenty-four on algebra and trigonometry (of which three are on complex numbers and four are revision of the second year) presumes a standard which is a good deal higher than the average throughout the country. The same tendency was noticeable in the first two volumes but it is more pronounced in this one and the student is expected to acquire in this course a better working knowledge of the calculus than the university Intermediate student who has a far better knowledge of fundamentals. The introduction, in the second volume, of graphical examples leading to the calculus was a sound step but it should have been accompanied by the postponement of some algebra and trigonometry to the third year. This would have avoided overcrowding the second year and necessitated cutting down the calculus in the third year, but surely it is sufficient in this year if the student masters the differentiation and integration of powers of x, $\sin x$ and $\cos x$ and their applications.

At the end of the book there are three test papers of national certificate standard, a complete set of answers to all the examples including the test papers, and eight useful pages of mathematical formulae.

H. V. LOWRY.

Continued Fractions*. (In Russian.) By A. Y. KHINCHIN. Pp. 104. 1936. Roubles 1.30. (Moscow)

Modern Russian textbooks on mathematics for secondary as well as those for high schools ignore almost completely all the questions which belong to the theory of continued fractions. Meanwhile the apparatus of these fractions, being one of the most powerful tools in many branches of mathematics (in the theory of numbers, the theory of probability, theoretical mechanics and cal-

* The Editor is indebted to Mr. Highdooc for offering this account of a modern Russian treatise on mathematics in the hope that it may interest those readers of the *Gazette* who would like to know something of the progress of mathematics in the U.S.S.R.

culus), should be mastered by every student intending to specialise in any of those branches.

The present monograph, which has been written in a masterly way by Dr. A. Y. Khinchin, a well-known professor in the Moscow University, is devoted to fill up the mentioned gap in Russian mathematical literature.

The book is divided into three chapters. The first chapter deals with the formal structure of the algorithm of continued fractions—that is, with such of their properties which do not depend on the assumption that the elements of fraction are integers. This anticipated discussion of all the formal points enables the author to treat further the essentially arithmetical part of the subject without making any digression because of formal considerations. The second chapter, entitled " The Representation of Numbers by Continued Fractions ", gives a good account of the subject treated in it, the fundamental importance of continued fractions by studying the arithmetical properties of irrationalities being particularly emphasised. The material of the mentioned two chapters gives all the necessaries for the applications of the theory and does not demand from the reader any special knowledge save some acquaintance with infinite series.

The contents of the third and final chapter is devoted to the more modern and more advanced questions of the theory, viz. to the fundamentals and the simplest applications of the *metrical theory* of continued fractions. The latter theory may be considered as a natural introduction to the so-called *metrical arithmetic of continuum* whose object is to determine the measures of sets of real numbers possessing any given arithmetical property (for instance, the property to admit a certain approximation by rational fractions). In spite of its juvenility the metrical arithmetic numbers already a lot of profound and elegant results (some of them may appear surprising at first sight) and, combining the ideas of such popular branches of mathematics as the theory of numbers and the metrical theory of sets of points, it cannot fail to be of interest for a rather wide circle of students. Therefore, the initiative of the author who gives for the first time in world literature such a comprehensive introduction to the new subject, is to be warmly welcomed.

To understand the contents of the third chapter the reader must have some knowledge of the theory of sets and calculus.

The reviewer is sure that Prof. Khinchin's original and stimulating book (the only fault of which is the lack of bibliography) will find many readers—and perhaps not only among Russians. G. H.

Heaviside's Operational Calculus as applied to Engineering and Physics. By E. J. Berg. 2nd edition. Pp. xv, 258. 18s. Electrical engineering texts. (McGraw-Hill)

Professor Berg intends his book mainly for electrical engineers; it would, however, serve to introduce the Heaviside operator to anyone who fears to tackle the more satisfactory but more severe expositions depending on Bromwich's contour integrals or Carson's infinite integrals and integral equations. There are twenty-seven short chapters taking the reader from the simplest kind of electric circuit as far as elementary cable problems; there is a long chapter on Graeffe's method of computing roots of an equation, for use with the expansion theorem, a chapter containing a list of formulae, and an account of Heaviside's work by B. A. Behrend, reprinted from an American technical journal. The main new matter in the second edition appears in an appendix of forty-four pages containing additional problems and solutions. The pace is easy, the engineer need not be frightened by the mathematics nor need the mathematician fear the technicalities, since these are reduced to the bare mini-

mum needed to set up the differential equations. It is evidence of the usefulness of the book that a German translation of the first edition appeared in 1932.

In general, Professor Berg's exposition seems admirably suited to its purpose, but at two points it might be improved. Considering the gentle pace of the book, the general expansion theorem comes in a little hastily at p. 15; it would have been better to have prepared the ground more thoroughly by working out one or two additional simple problems in the text. The other point concerns the first mention of Heaviside's p itself. This is made in an off-hand way on p. 3. The first page is occupied with prefatory remarks, p. 2 mentions $D \equiv d/dt$, Heaviside's unit function 1 which is unity for positive t and zero for negative t, and puts down the equation for the current i in a simple inductive circuit, namely,

$$L di/dt + ir = E1.$$

Then on p. 3 comes the first mention of p. "It will be evident later that there is an advantage in introducing the symbol p for the operator d/dt. As a matter of fact, the operator p does all that d/dt can do and more. Introducing p in the equation given above, we get

$$E1 = ir + Lpi. \ldots"$$

At this stage the novice is likely to ask whether p is d/dt or not. If not, what is it? If it is, how can d/dt called p take care of the initial conditions when d/dt called D can not? Of course, he will soon realise that the answer lies in the use of the unit function, but it seems likely that he will feel that there is a good deal of mystery about the whole procedure. Much of this mystery can be cleared away by making the fundamental operation that of integration between limits,

$$p^{-1}.(\quad) = \int_0^t (\quad) dt.*$$

Heaviside's methods deserve more attention in this country than they appear to receive; this book can be recommended to those who wish to acquire a working knowledge of the manipulative side of the subject.

T. A. A. B.

Leçons d'Algèbre et de Géométrie. III. Élimination. Éléments de Géométrie réglée. Transformations de Lie. Applications à la Géométrie conforme. By R. Garnier. Pp. vi, 280. 80 fr. 1937. (Gauthier-Villars)

The second volume of this book dealt with matters more or less familiar to the student at the Scholarship stage or taking a Pass or Honours degree course, namely conics and quadrics. But with the third volume we pass to material with which the Honours student whose bias is not geometrical is likely to have only a nominal acquaintance. It is easy enough to attack the tendency for students to be taught a great deal more about a good deal less, and easy enough to defend it; the practical step seems to be the recommendation of books for private reading where matters which the student ought to know are treated by methods which will be congenial to his particular bent.

Prof. Garnier starts with a chapter on symmetric functions and elimination, and with this groundwork of classical, manipulative algebra, he gives a careful algebraic account of the idea of a geometrical locus, and proceeds to study matters about which every mathematician ought to know something, if not necessarily a great deal; Plücker's line coordinates, the linear complex, the tetra-

* Some discussion of this point, and of much else relating to the subject, will be found in articles by Carslaw, Jeffreys and Bromwich in Vol. XIV of the *Gazette*.

hedral complex, and Lie's transformation. The final chapter, entitled "Les transformations de contact conservant les lignes de courbure", may appear as likely to interest only the specialist, but it leads very naturally and interestingly to the geometric side of the theory of automorphic functions. A geometer might suggest that his subject is being treated merely as a convenient field for the application of algebra, much as the Cambridge mathematicians of the nineteenth century were apt to treat the universe as a convenient field for the application of Tripos mathematics; those who are algebraists but who would like to learn some geometry in a way which suits their own inclinations will find this volume well worth reading. The author writes in the standard French mathematical style, so that reading is easy. Occasionally the algebraic bias makes for dullness, but this is compensated by the sense of precision not always imparted by geometrical treatises. The printing is, of course, of the usual Gauthier-Villars excellence. T. A. A. B.

Höhere Mathematik für Mathematiker, Physiker und Ingenieure. By R. Rothe. Teil IV, Heft 4. Pp. 51-106. RM. 1.80. 1937. Teubners mathematische Leitfäden, 36. (Teubner)

In this part of Dr. Rothe's treatise we have the solutions to examples on infinite series, integrals depending on a parameter, and vector calculus. This forms a valuable supplement to a work which is one of the most useful and satisfactory textbooks on analysis I have ever met. T. A. A. B.

Men of Mathematics. By E. T. Bell. Pp. 653. 12s. 6d. 1937. (Gollancz).

Every school library, every young mathematical enthusiast, every teacher of mathematics ought to get this book. I cannot imagine that anyone who opens it will fail to read any word of it. There are numerous errors of detail, unimportant in a stimulant; there is a trace, it seems to me, of an error in principle; but the author succeeds in his main aim, to portray mathematicians three-dimensionally by setting them in or against their historical background, social, cultural and philosophical. After an introductory chapter, there is a chapter on Zeno, Eudoxus and Archimedes, the three Greeks for whom the author has a good word to say, and then we make a leap to the "century of genius" and its splendid galaxy, Descartes, Fermat, Pascal, Newton, Leibniz, and then on until the end comes in Chapter XXIX, "Paradise Lost", with Georg Cantor as Eve. Professor Bell claims that a secondary or public school course in mathematics is sufficient to enable the book to be read with understanding, and this claim is sound, though it would mean that certain sections would have to be read very thoroughly and carefully. The style has something of the exuberance we associate with California, and so may serve as an added stimulus (or even as an irritant). The picture of Galois spending the night before his fatal duel in putting down the outlines of his great discoveries, with the frequent marginal phrase, "I have no time," might be even more moving if it were drawn with greater restraint; yet Professor Bell's righteous anger commands our sympathies, and in this case the sub-heading to the chapter is well chosen: "Against stupidity the gods themselves fight unvictorious."

It may be worth a little space to discuss in some detail what appears to me to be an error in principle, the effects of which can be seen in several chapters. We can never hope to see men of another age as they really were, but too often Professor Bell sees them entirely with twentieth century eyes. His accounts of mathematical work seem to be, as far as I know, accurate and impartial; but he is too fond of the historical "ought-to-have-beens". For instance, to him Newton's work at the Mint is simply a "degradation of the supreme intellect of the ages". "Had his officious friends but left him alone, Newton might

easily have created the calculus of variations." But let us suppose the currency unreformed; historians tell us that the consequences to England would have been worse than the loss of a pitched battle; they might then have entailed the defeat of the League of Augsburg, the restoration of the Stuarts. Where would the Whig Newton have been? Certainly in prison, probably to die of gaol-fever. If this is absurd, it is no more absurd than the complaint that Newton was Newton and not the man the twentieth century may think he ought to have been. Besides, Professor Bell overlooks the way in which men of the seventeenth century thought about the service of the state. "Study the mathematics and cosmography. These fit for public services for which a man is born." So wrote the great English man of action in that century; is this not the same spirit in which Newton said "I do not love to be thought by our own people to be trifling away my time . . . when I am about the King's business"? "In an age of corrupt and self-seeking and treacherous politicians", says Professor G. M. Trevelyan, "a great financier was struggling in war-time with the terrible problem of the re-coinage, and called in the aid of this great mathematician and scientist to give a few years 'to the King's business'. Is not that a record for a country to be proud of?" There is no need to set Newton up as the perfect man; but I would urge that in this instance there is as much evidence for the suggestion that Newton was doing what he believed to be his duty as there is for Professor Bell's picture of him "pursuing popular esteem", seeking "crudely and directly" to elevate himself into a social class above that in which he was born.* I have chosen this episode because of its familiarity: but it could be argued that the same fault in historical perspective is responsible for the view of Pascal as a morbidly religious pervert; for the remark that "Hamilton's deepest tragedy was . . . his obstinate belief that quaternions held the key to the mathematics of the physical universe" when we all know that vector systems "are being swept aside by the incomparably simpler and more general tensor analysis"; for the contrast, "compared to what glorious Greece did in mathematics the nineteenth century is a bonfire beside a penny candle", unless it is certain that the author realises the advantages upon occasion of the penny candle.

If I have said too much about what may be only a difference in point of view, it is because the book provokes thought and encourages argument. To all, mathematicians or not, who wish to see these great names as men, I can recommend this volume with full confidence that every page of it will be found interesting and informative. T. A. A. B.

* Evidence now available seems to show that Newton himself applied for the Mint; but it does not throw much light, it seems to me, on his motive for doing so.

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